THURSDAY, JANUARY 28, 1892.

THE ASTRONOMICAL THEORY OF THE GLACIAL PERIOD.

The Cause of an Ice Age. By Sir Robert Ball, Astronomer-Royal for Ireland. Pp. 180. (London: Kegan Paul and Co., 1891.)

THIS book gives a popular account of the theory of Adhémar and Croll as to the causes of glacial periods in geological history.

The author's power as a popular expositor is well known, and this little book shows him at his best. He knows when to drive a point home, and yet is never tedious in his reiteration But he has given here something more than a lucid explanation, for he makes a valuable contribution to the subject, and the book may be read with advantage by those who are already acquainted with the literature bearing on the theory.

The theory itself may be sketched in outline as follows:—

It is known that, under the perturbations of Venus and Jupiter, the eccentricity of the earth's orbit varies within certain limits. When the eccentricity is large, and when the precession of the equinoxes brings the perihelion to near the middle of, say, the northern winter, the annual supply of solar heat is so distributed that there will be a glacial period in the northern and a mild climate in the southern hemisphere. Two or three maxima of glaciation and mildness will usually succeed one another at intervals of 10,500 years, because the eccentricity varies with extreme slowness. When the eccentricity is small, as at present, a moderate climate will prevail in both hemispheres, whatever be the position of the perihelion.

The keynote of Sir Robert Ball's presentation of this theory is given in a short mathematical appendix. I am disposed to dissent to some extent from the manner in which this view is set forth, but the general argument will, I think, do much to convince the scientific world of the truth of the theory, even where Croll's more elaborate discussions failed to do so.

I will now give a paraphrase of the argument, and will point out where it appears to me open to objection.

The time taken by the earth to describe a degree of longitude round the sun varies as the square of its distance from the sun, and the intensity of solar radiation varies inversely as the square of the same distance. Hence the amount of heat received by the whole earth during the description of a degree of longitude is constant.

Let the year be divided into only two seasons, viz. the northern summer or southern winter when the sun is north of the line, and the northern winter or southern summer when the sun is south of the line. Also let similar days in summer and winter be defined as days on which the sun sets (say at Greenwich) as much after 6 p.m. as before 6 p.m.; similar parts of summer and winter will mean parts limited by similar days.

Now consider the solar heat incident on any specified area of one hemisphere, during any specified portion of the summer and during the similar portion of the winter. Suppose that the heat incident on the area in the portion of summer added to that incident on it during the similar portion of winter be denoted by 2, and suppose that the excess of the heat incident in the portion of summer above that incident in the similar portion of winter be denoted by $2a$; then it is clear that $1 + a$ is proportional to the amount of heat received by the specified area during the specified portion of its summer, and $1 - a$ is proportional to the amount of heat received by the area during the similar portion of winter.[1] Thus we may say that the contrast between the summer and winter supplies of heat (for given area and given portions of summer and winter) is represented by the fraction $(1 + a) \div (1 - a)$.

This is, of course, equally true when the whole hemisphere, and the whole of summer and winter, are considered, and Sir Robert Ball shows that a is then equal to $2 \sin 23° 27' \div \pi$; $(1 + a) \div (1 - a)$ is found to be almost exactly as 5 to 3. Using percentages he gives the ratio as 63 to 37, but the simple numbers 5 to 3 afford a closer approximation to accuracy.

It is clear that if the specified portions of summer and winter embrace the solstices, and if the specified area is tropical, a will be small, and if it is polar it will be large. The fraction $(1 + a) \div (1 - a)$ continually increases as we go northward, and it may be taken as a measure of the severity of a climate. It is quite uncertain how far the climate of any one place depends on the heat supplies of the whole hemisphere on which it lies, and therefore it is uncertain how large an area and how long a season we ought to take into consideration in the present investigation. But I should have thought it legitimate, in treating of the causes of glaciation, only to consider the semi-annual heat supply of a polar cap, comprising, say, all the area north of latitude 30°; thus would have made $(1 + a) \div (1 - a)$ much greater than 5 to 3. It does not seem to me, however, that we are bound to find an answer to this almost insoluble problem.

So far we have considered the supply of heat whilst the earth describes so many degrees of longitude round the sun, but climate depends on the supply of heat during a given time.

When the earth's orbit is circular, summer and winter are of equal length, and so also are similar portions of summer and winter; thus the two ways of estimating the heat supply coalesce, and the contrast between the summer and winter *daily* supplies of heat is also represented by the fraction $(1 + a) \div (1 - a)$. The present condition of affairs differs but little from this standard case, and we know that the contrast between the summer and winter daily supplies of heat is such as to produce certain known climates, differing according to latitude.

[1] If $\frac{1}{2}\pi \pm \phi$ be the sun's hour angle at sunset on any day in summer, and on the corresponding day in winter, and if the sun's parallax on those days be proportional to $1 \pm E$, then it is easy to show that the amount of heat received by unit area in the course of the day is proportional to

$$(1 \pm E)^2[(\phi + \cot \phi) \pm \tfrac{1}{2}\pi] \sin \delta \sin \lambda,$$

where $\pm \delta$ is the sun's declination, + in summer and − in winter, and λ is the latitude of the place of observation.

It follows that, what is called in the text, the contrast for unit area in latitude λ, for this pair of days is—

$$\frac{1 + a}{1 - a} \cdot \left(\frac{1 + E}{1 - E}\right)^2, \text{ where } a = \frac{\pi}{2(\phi + \cot \phi)}.$$

The expression for the heat supply on unit area during any portion of summer or winter involves elliptic integrals, which might be given if it were worth while.

A triple integral is required to express the heat supply of any specified area during any specified portion of the year.

The question we have to ask is, If the orbit becomes eccentric, how will the contrast of daily supplies be affected?

In order to answer this, let us go at once to the extreme, when the eccentricity of orbit is a maximum. We learn that if aphelion is at midsummer, summer will be 199 days, and winter 166 days; and the converse is true when the perihelion is at midsummer.

Since 199 is to 166 nearly as 6 to 5, we see that with midsummer perihelion there are 5 days of summer to 6 of winter, and with midsummer aphelion there are 6 of summer to 5 of winter.

Hence, with midsummer perihelion, the short summer daily supply of heat may be taken as proportional to $\frac{1}{5}(1 + a)$, and the long winter daily supply as proportional to $\frac{1}{6}(1 - a)$. Hence the contrast between the short summer and long winter daily supplies is represented by $\frac{6(1+a)}{5(1-a)}$; that is to say, the standard contrast is augmented in the ratio of 6 to 5. Next, with midsummer aphelion, the long summer daily supply of heat may be taken as proportional to $\frac{1}{6}(1 + a)$, and the short winter daily supply as proportional to $\frac{1}{5}(1 - a)$. Hence the contrast between the long summer and short winter daily supplies is represented by $\frac{5(1+a)}{6(1-a)}$; that is to say, the standard contrast is diminished in the ratio of 5 to 6.

In the first case, the heat supply is less evenly distributed through the year than at present, and we have a much more severe climate; in the second, it is more evenly distributed, and we have a much milder one. It follows also that, if we compare the two extreme cases together instead of both with the mean case, the change of contrast is represented by the ratio of 6^2 to 5^2, or of 36 to 25.

I must refer the reader to the able discussion in the book of the effects which we have reason to suppose would flow from a change of contrast represented by the numbers 36 to 25; and it must suffice to say here that it seems enough to explain on the one hand the occurrence of the glaciation of England, and on the other hand the occurrence of sub-tropical plants in Greenland.

Now, the above seems to me to be substantially the argument in the book, but I dissent from the stress laid on the numerical determination of the quantity a. On p. 90 Sir Robert says:—

"This theory will be entirely misunderstood unless the facts signified by these numbers (the evaluation of $(1 + a) \div (1 - a)$) are borne in mind. No one can discuss the astronomical theory of the Ice Age unless the figures 63 and 37 (5 and 3 are more accurate) form a portion of his consciousness, and the refrain of his every argument."

It may be admitted that it might have been more difficult to present the argument in a popular form without assigning a numerical value to a, but Sir Robert Ball is fully equal to such a task; and I contend that the numerical value of a is beside the mark, even if a value, appropriate to the investigation in hand, were attainable.

After presenting his own view of the question, Sir Robert Ball says (p. 134) that Croll does not seem to have been really aware of the full strength of the astronomical theory, and in this I entirely agree. Croll, in fact, rather weakens than strengthens his position when he tries to trace in detail the action and reaction of the astronomical cause, for in doing so he is led to maintain various theses which are not susceptible of proof, and are even highly doubtful. He thus takes as the central point of his position one at which it appears to me to be weakest. In 1886 I wrote:—

"Adequate criticism of Mr. Croll's views is a matter of great difficulty, on account of the diversity of causes which are said to co-operate in the glaciation. In the case of an effect arising from a number of causes, each of which contributes its share, it is obvious that if the amount of each cause and of each effect is largely conjectural, the uncertainty of the total result is by no means to be measured by the uncertainty of each item, but is enormously augmented. Without going far into details, it may be said that these various concurrent causes result in one fundamental proposition with regard to climate, which must be regarded as the keystone of the whole argument. That proposition amounts to this—that climate is unstable.

"Mr. Croll holds that the various causes of change of climate operate *inter se* in such a way as to augment their several efficiencies. Thus, the trade-winds are driven by the difference of temperature between the frigid and torrid zones, and if from the astronomical cause the northern hemisphere becomes cooler, the trade-winds on that hemisphere encroach on those of the other, and the part of the warm oceanic current, which formerly flowed into the cold north zone, will be diverted into the southern hemisphere.[1] Thus the cold of the northern hemisphere is augmented, and this in its turn displaces the trade-winds further, and this again acts on the ocean currents, and so on; and this is neither more nor less than instability.

"But, if climate be unstable, and if from some of those temporary causes, for which no reasons can as yet be assigned, there occurs a short period of cold, then surely some even infinitesimal portion of the second link in the chain of causation must exist; and this should proceed, as in the first case, to augment the departure from the original condition, and the climate must change."[2]

I see no reason to depart from what I said five years ago, but I now learn from this book how it is that Croll mistook the strong points of his own theory, and that a more forcible proof of it may be contained in a short work than in an elaborate volume. After expressing this opinion, it is but fair to quote and indorse the following passage (p. 112) on Croll's famous work on "Climate and Time":—

"I was greatly struck," says the author, "by this work when I first read it many years ago. Subsequent acquaintance with this volume . . . has only increased my respect for the author's scientific sagacity, and my admiration for the patience and the skill with which he has collected and marshalled the evidence for the theory that he has urged so forcibly."

There are a few other points in the "Ice Age," not involved in the main line of argument, on which I should like to comment.

The method adopted of stating the disturbing forces of the planets on the earth appears to me unduly sensational. We learn (p. 74) that the disturbing force of Venus is 130 million million tons, and it is impossible not to be impressed with the magnitude of the force. But if we had been told that the disturbing force on each pound of the earth's mass was only 1/7000 of a grain,

1 Ball (p. 134) fails to see the force of this argument.
2 Brit. Assoc. Report, 1886, Address to Section A.

we should have been equally impressed with its insignificance—and yet the two statements are virtually the same. In fact, the unscientific reader is not likely to realize the prodigious number of pounds in the earth's mass.

It may be remembered that Croll computes, in "Climate and Time," the value of the eccentricity of the earth's orbit from Leverrier's formulæ, and endeavours thus to assign actual dates to various glacial periods. Now, Sir Robert Ball very justly will not admit that our knowledge of the solar system is accurate enough to justify the application of these formulæ to the enormously long intervals of time involved. I think, however, that it would have been of interest to the general reader to be told in round numbers the kind of intervals which we have reason to believe may have elapsed between one glacial period and the next; in fact, to learn whether the intervals are probably millions of millions of years, or hundreds of thousands of years. I conjecture that our knowledge of the planetary movements is sufficient to enable us to say that such an interval may be something comparable with 200,000 years. I should like, further, also to ask Sir Robert Ball whether he does not consider that Leverrier's formulæ may probably be relied on to give at least a rough approximation for about 100,000 years in the past; and, if this is so, whether we might not conclude, with fair probability, that the last glacial period occurred about that number of years ago? I must, however, disclaim any special knowledge on this point, and I should gladly see his opinion, or that of any other physical astronomer, on the matter.

In conclusion, I wish to say that, in making the foregoing criticisms and suggestions, I have no intention of disparaging the book; on the contrary, it is only because it is a good book that it is worth while to consider it carefully. I have found it profoundly interesting from end to end, and I am convinced that it will be widely read, as it deserves to be.

G. H. DARWIN.

POPULAR ZOOLOGY.

Animal Sketches. By C. Lloyd Morgan, F.G.S., Principal of University College, Bristol. (London: Edward Arnold.)

THIS is one of those delightful books of natural history for young people which their parents never had the benefit of, and for which they ought to be duly thankful. A competent naturalist here gives them the result of his full and varied knowledge, but gives it so blended with imagination and humour, so intermingled with anecdote and personal adventure or observation, as to make it a real story-book about animals, by reading which we learn much of their lives and habits, their peculiarities of structure and their relations to each other, while we seem to be only reading for amusement. There is nothing systematic in this volume. It is merely a collection of miscellaneous chapters on a variety of animals, beginning with the lion and ending with the oyster, every chapter of which is both pleasant and instructive.

The best way to notice a book of this kind is to give a few examples of the author's style, which in this case will certainly commend the book better than any description of its contents. First, then, as a bit of serious biology, we will give a passage on the nesting-habits of the ostrich.

"The nest is scooped out in the sand, and two or three hen-birds may combine to lay their eggs in it, to the number of about twenty. It is said, and that by several observers, that, besides the eggs laid in the nest, each hen lays several in the neighbourhood, and that these are broken when the young are hatched, and the contents are given them as food. But I am inclined to regard these statements with some suspicion. The hens take turns in sitting during the day, never leaving them long in the scorching heat of the South African sun. But at sun-down the cock-bird takes charge of the eggs, and sits throughout the night. He is not going to be bound by any conventional rules as to the proper division of labour between the sexes.

"A very careful observer, Mrs. Barber, has drawn attention to the fact that the indistinct grey colours of the hen ostrich are wonderfully adapted for purposes of concealment. These birds while upon their nests do not erect their necks, but place them at full length in front of them upon the ground; and the grey-brown body might, Mrs. Barber says, be easily mistaken for some other object, such as, for instance, an ant-hill, so common on the plains of South Africa. That so large a bird should be inconspicuous may seem surprising; but another observer, Mr. W. Larden, tells us of his experience with the rhea, or South American ostrich, which seems quite to bear this out. 'One day,' he says, 'I came across a rhea in a nest that it had made in the dry weeds and grass. Its wings and feathers were loosely arranged, and looked not unlike a heap of dry grass; at any rate the bird did not attract my attention until I was close on him. The long neck was stretched out close along the ground, the crest feathers were flattened, and an appalling hiss greeted my approach. It was a pardonable mistake if for a moment I thought I had come across a huge snake, and sprang back hastily under this impression.'

"The male ostrich, with his splendid black and white feathers, would not be thus inconspicuous *by day*. But he sits at night, and his strength and pugnacity would induce most other creatures to let him alone. Mrs. Barber describes the careful manner in which the female bird approaches the nest in the morning, when her turn for incubation has come. In wide circles, and apparently in the most unconcerned manner, she will feed round the nest, never once looking towards it, but gradually approaching nearer and nearer to it by diminishing each circle as she walks round, until at length her perambulations have brought her to within a yard or so of the nest, when the birds will rapidly change places, the male walking swiftly away, and not remaining in the vicinity of the nest during the day. The wonderful rapidity with which the change is effected is perfectly astonishing, and it is impossible to see the exact manner in which it is done, so swiftly do they change places."

As an example of Mr. Lloyd Morgan's lighter manner, what can be more attractive than the opening sentences of his chapter entitled "Long-nose, Long-neck, and Stumpy"?

"'And which of all the animals in the Zoo do you like best?' I said to a bright, fair-haired little girl whom I had assisted in her descent from the elephant.

"'I think I like Long-nose, Long-neck, and Stumpy best, because they are so big and curous, and Long-nose best of all because he has given me a ride. Did *you* know it was his nose?'

"Of course I affected the most extreme surprise and

delight at the novel suggestion that the big, patient animal's trunk was really his nose, and said that I had always thought it was his proboscis.

"'No, it isn't that, it's his nose. Auntie says so. That's Auntie over there, waiting for me. I suppose you's seen Stumpy?'

"I inquired who Stumpy was, and whether I might not know him by another name.

"'I think they sometimes call him Pottums. But we call him Stumpy. Now I must go to Auntie.'"

And then our author tells us much about those three strange and remote types, the elephant, hippopotamus, and giraffe, in his own pleasant manner—their singular structure and habits, their external diversities concealing so much internal resemblance—devoting, however, most attention to the elephant, and correcting some exaggerated statements that have been made respecting that animal.

One of the most interesting chapters is that on snakes. It is full of information, and there is an almost fascinating account of the whole process of capturing and devouring its prey by a python, as observed at the Antwerp Zoological Gardens. Prof. Lloyd Morgan has visited, or lived in, many lands, and often enlivens his pages with personal anecdotes, of which the following is by no means the most remarkable:—

"My first experience of South African death-dealing snakes was somewhat different. One of my pupils brought me, in a large cigar-box, a 'ring-hals-slang,' a deadly and courageous snake not uncommon at the Cape, and turned him out on the verandah for our delectation. He was a spiteful little fellow, with an ominous hood, dark glossy skin, and glistening brown eye. He struck viciously at the cigar-box held up before him, indenting the wood, and moistening it with venom and saliva. I was particularly anxious to dissect out the poison-gland and examine the poison-fang of the snake, so my friend kindly presented it to me, replacing it in the cigar-box, which he tied securely. After examining the fastenings, I placed the box on the window-sill of my bedroom, which looked out into the verandah, and left it there for the night. Next morning I procured a large washing-pan, big enough to drown a small python, placed the cigar-box therein, loaded it with a couple of bricks, and poured in water to the brim. I gave the 'ring-hals' three good hours to get thoroughly drowned, removed the bricks, took out the box, gently cut the string, lifted the lid—and found that I had been drowning with the utmost care an empty cigar-box. It had been securely tied, and how a creature more than thrice the girth of my thumb had managed to escape was, and still is, a mystery to me.

"I leave the reader to imagine the detailed search of every cranny of our bedroom, on which my wife insisted. For several days every boot had to be hammered with a stick before it was put on; I stood on a chair and shook every pair of trousers, and other analogous garments, lest they should be already occupied. But no 'ring-hals' was forthcoming. And I suppose it must have been a week or so afterwards that I was summoned to the kitchen to expel an unwelcome intruder—the black cook being, so far as her skin permitted, pale with terror—which proved to be none other than the missing 'ring-hals.' I despatched him promptly, but not by drowning."

Among the specially good chapters are those on "Cousin Sarah," the chimpanzee; on the sparrow as typical of birds, under the title "Master Impertinence"; on chameleons, frogs, sticklebats, crayfish—but it is useless to particularize when all are good. The book is well illustrated, both with pictures and diagrams; and we may especially note that the structure of the elephant's tooth and that of the bee's compound eye are clearly elucidated by the cuts that accompany the descriptions.

Lastly, there is a pervading tone of sympathy with all that lives, as well as a general love and admiration of Nature, that renders it a most suitable work for the young. The cover and general get-up are attractive, and every school should add this charming volume to its list of prizes, with the certainty that it will be highly appreciated for its own sake by the recipients, and that its influence will be altogether wholesome and good.

A. R. W.

PHYSIOLOGICAL CHEMISTRY FOR MEDICAL STUDENTS.

Outlines of Practical Physiological Chemistry. By F. Charles Larkin, F.R.C.S., and Randle Leigh, M.B., B.Sc. Second Edition. (London: H. K. Lewis, 1891.)

THE authors state in their preface that this edition of the work is "the result of seven years' experience in teaching the subject to medical students," from which we gather that the medical student is being treated in the physiological laboratory in much the same spirit as he has long been dealt with in that of the chemist. The work before us is constructed upon an essentially similar principle to those numerous little treatises, the be-all and the end-all of which is to instruct the medical student in three months how to analyze simple salts. For such treatises, and the unedifying kind of instruction to which they give rise, neither teacher nor student is to be blamed: the fault lies with the authorities who frame the medical curriculum and the syllabus for the subjects of examination. The root of the mischief lies in having to treat the medical student during his preliminary scientific training as a separate genus from the student of general science, a course which is rendered necessary through the attempt to crowd such a large number of subjects into a period of time which is wholly inadequate for the purpose; whilst another evil tending to degrade the standard of the examinations is the existence of competing corporate bodies possessing the power of granting medical qualifications. For these ills the obvious remedies are, on the one hand, extension of the minimum time occupied by the curriculum, whilst, on the other hand, a uniform standard for qualification is required for the whole of the United Kingdom: fortunately, both of these changes are already in progress. Considering the necessarily technical and empirical character of the greater part of medical education proper, it is, in our opinion, of the greatest importance that in the teaching of the pure sciences to medical students there should be as little empiricism and rule-of-thumb as possible; and it is, therefore, just in his study of chemistry that the future physician and surgeon should receive an insight into the scientific use of the understanding.

Now, it is in this respect that the work before us, which contains a large number of facts arranged in a handy form, falls short of what is required. The subject of physiological chemistry is still at best such a very empirical one, that it becomes the more necessary to give an

explanation of phenomena whenever they are properly understood. It will be urged by some that such information is out of place in a practical manual, and that it belongs to theoretical works and lectures on the subject; in our experience, however, the great difficulty in laboratory teaching is to make the student associate his practical work with what he hears in the lecture-room or reads in his study, and that it is only by continually drawing his attention to the bearing of his experiments that the latter are made to have any great educational value. In short, it is only in this way that the instruction in a chemical laboratory materially differs from that obtainable in a kitchen, and that a work on practical physiological chemistry will be raised above the level of a hand-book on cookery. By amplifying their work on the lines indicated, we believe that its value to the student would be much enhanced, for in its present form it can only be used to much purpose under the guidance of an accomplished and energetic demonstrator.

In conclusion, we must point out an error which should hardly occur in a work professing to be the result of experience, still less in a second edition. On p. 29 the student is told to prepare lactic acid as follows:—"Place 50 c.c. of milk in a warm chamber for several weeks until it becomes strongly acid. Shake the ether, and decant the ethereal extract. Evaporate the ether, add extract residue with water. It is strongly acid, and yields crystals [*sic*] of lactic acid." The passage obviously contains several printers' errors, but the crystalline nature of lactic acid is new to us.

OUR BOOK SHELF.

Problems in Chemical Arithmetic. By E. J. Cox, F.C.S. Pp. 76. (London: Percival and Co., 1891.)

THIS book contains a series of arithmetical examples chosen to meet the requirements of the examinations held by the Science and Art Department in the elementary stage of chemistry. There does not seem to be any outstanding feature to distinguish the book from others of its kind; indeed, setting aside the actual exercises, which may be useful to the teacher, the explanations of the principles involved in the calculations are, as a rule, meagre, and frequently inaccurate. Information such as the following is, to say the least of it, faulty: "Whatever may be the weight of any given volume of water, an equal volume of mercury under similar conditions will be 13·6 times as heavy." This conclusion was made to follow as a result of an apparently practical method of obtaining specific gravity, although no mention whatever was made of temperature or its effects in the description of the process.

The author keeps on repeating, without any qualifying clause, that the formula of a compound represents the molecule of the compound; and the student is led to infer (pp. 19 and 20) that the empirical formula as found by analysis serves to fix molecular weight. As a consequence of the above idea, several problems are set—such as, Find the molecular weight of starch, of fluorspar, &c.—which are misleading, since they cannot be solved in practice.

The relationship between vapour-density and molecular weight is given, but the use of the former as a means of determining the latter is not even hinted at; and the author prefers to find the value of the ratio, molecular weight to vapour-density, by the use of numerical examples, rather than by a general process of reasoning, although all the necessary points have previously been stated.

The examples include a series selected from the Department examination papers, and a table of contents and list of answers are included.

If the working of elementary problems in chemistry is to be an intellectual process, founded upon an appreciation of fact and theory, which may be supplemented but has not to be corrected by the student as he progresses, books such as the above fail to fulfil this end.

LETTERS TO THE EDITOR.

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The Theory of Solutions.

IN NATURE of December 31 (p. 193) occurs a review by "J. W. R." of my book on "Solutions," which gives me occasion to enter, in a few words, upon the questions there brought forward, and to set right some errors, which have recently appeared in other places as well.

First of all I wish to express to Mr. J. W. R. my cordial thanks for the thorough and careful manner in which he has made himself acquainted with the contents of my book. I have no intention to discuss the objections made, some of which I am quite willing to recognize as well founded, but to make clear one important question in which I do not seem to be properly understood by my critic.

Mr. J. W. R. begins his discussion with the words, "To the fundamental question—'Is solution a physical or a chemical process?'—the answers are various"; and out of this variety he evidently finds against me a reproach. I have intentionally neither set up this question nor sought to answer it, for I hold it to be unclear and therefore very harmful. To the question, "Is gas-formation a chemical or a physical process?" would be answered, "In certain cases, as in the development of carbon dioxide out of champagne, a physical one; in others, as the development of carbon dioxide from limestone, a chemical one; and in many cases, as in the development of hydrogen from palladium hydride, one would be in doubt what to answer." The question set up is faulty in implicitly assuming that solution must be either a physical or a chemical process, and by this prepossession he is hindered from recognizing that I was entirely justified in placing the physical or chemical side of the question in the foreground according to the nature of the case. However, in case Mr. J. W. R. is not satisfied with this explanation, and insists upon setting up this question, I must postpone further discussion upon it until he shall have given me a sufficient definition of the ideas "physical" and "chemical" processes, and of their distinction. I know of no such definition, and have consequently not made use of the expressions.

From the definition of *solution* given by me, Mr. J. W. R. concludes that I am a representative of the "physical theory" of solutions, in contrast to which he places the "chemical theory." I cannot repeat energetically enough that I have never recognized such a contrast, and that I cannot at all admit the existence of such a contrast. It has never been maintained, either by me or by any other representative of the newer theory of solutions, that no interaction takes place between the solvent and the dissolved substance; on the contrary, I have for years directly encouraged research work directed towards making clear the nature of such interactions. What distinguishes the new theory of solutions, founded by van 't Hoff, from the others is that it has succeeded in discovering and bringing into connection a series of properties of solutions, *which can be treated entirely independently of the question of a possible interaction between the parts of the dissolved substance and of the solvent.*

All these properties hang together with the fact that in the making of a solution or in the altering its concentration there is developed or absorbed a definite amount of free or available energy, which is equal for equimolecular quantities of different substances, and is independent of the nature of the solvent. The amount of this free energy is the same as in the analogous processes with gases. These are purely experimental facts,

which, so far as I see, are not questioned by Mr. J. W. R. The newer theory of solutions, in its entirety, is only a development of the consequences of these facts; and if errors are present therein, they can only be errors in the application, since the premisses are correct: for the proof of such errors we can, of course, in the interest of science, be only thankful.

It cannot possibly be used as an objection to the newer theory of solutions that it concerns itself at present with those properties of solutions which depend only so far upon the nature of the substance in question as one constant—the molecular weight—is concerned; and which properties, like the relation holding for gases between pressure, volume, and temperature, I have proposed to term *colligative* properties. For Mr. J. W. R. agrees with me that the number and variety of the conclusions which have been drawn from this fact of the existence of these properties is already very great, and it seems to be open to no doubt that the possible applications are by no means exhausted. It will certainly be the task of the future to take also into consideration those properties of solutions which depend upon the individual nature of substances; and this has, indeed, already to a certain extent been done in a particularly important case—that of the change with the solvent of the molecular weight of a dissolved substance, and especially that of the specific property of water to form electrolytic solutions. But I do not believe that we can be justly reproached for having endeavoured to first solve the relatively more simple problems before turning to the more complex ones. It will be here that what is termed by Mr. J. W. R. the "chemical theory" will take its proper place.

I beg Mr. J. W. R. to recall the history of the rivalry between these two "theories." Van 't Hoff and his successors developed the laws of solutions entirely without polemical strife, because, since the fundamental ideas of van 't Hoff's theory were entirely new, there was nothing at all in its territory to combat, as till then there was nothing there. The attacks upon van 't Hoff's theory were begun by an investigator who had until then directed his attention exclusively to the phenomena which I have above characterized as individual, and who was evidently unprepared to deal with such colligative properties. The defence had to consist in an unceasing clearing up of misconceptions. Now, the greatest of these misconceptions is, that both "theories" are rivals. The existence and form of the laws founded by van 't Hoff and his successors stand at present beyond question; if the totality of these laws be termed the physical theory of solutions (which I should not do), there is nothing to be objected to this. But what has until now been known as the hydrate theory has not been in a position to give any information whatever in regard to these laws; none of them have been discovered with its aid, and since it has for its subject not the colligative but the individual properties of solutions this will not be otherwise in the future. In fact, the existence of the colligative laws, or van 't Hoff laws, is entirely independent of whether hydrates exist in solutions or not, and all attempts, successful or otherwise, to demonstrate the existence and composition of such hydrates, lead conversely in no wise to the van 't Hoff laws.

It will possibly not be superfluous to emphasize that with the new theory of solutions the question is not one of hypotheses, but of facts, of numerical relations. Whether one can form a conception as to the cause of the colligative laws of solutions with the aid of the kinetic molecular hypothesis or in any other way, is, for the actual existence of these laws, just as much a matter of indifference as it is for the existence of the laws of gases. In my book the question is this one of facts, and although I have therein made more use of molecular considerations than I should at present hold to be proper, yet I have done so only to render more clear the actual relations, and never to prove quantitative laws.

The *theory of solutions* which I represent and defend consists, accordingly, of a certain number of laws, *i.e.* of exact relations between measurable quantities belonging to solutions. I cannot see that that which as "chemical theory" is set in opposition thereto contains anything similar to this. The latter contains in actual material certain methods—very doubtful ones to my mind—for demonstrating the existence of compounds between dissolved substance and solvent. The result of these endeavours remains, however, in any event, entirely without influence upon those colligative laws. The same may be said of the future answer to the question, whether an interaction, and what, occurs between dissolved substance and solvent. In order to render this apparent, I need only recall the fact, recently observed by Goldschmidt, that the depression of the freezing-point in a basic solvent, such as paratoluidine, caused by dissolving in it an acid, has practically the same value as that effected by the equal molecular quantity of an indifferent substance, although in the first case a chemical compound is formed and none in the second.

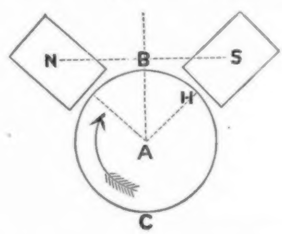
So, in the presentation of *laws* of solutions, as known up to the present, and which form the subject of my book, the so-called hydrate theory or chemical theory, did not enter into the question, because it had discovered no law of anything like general character. And further, that the methods and hypotheses of this "theory" cannot be yet looked upon as reasonably supported scientific results is known to the English-reading scientific public from a number of papers published in recent numbers of the *Philosophical Magazine*. W. OSTWALD.

Leipzig, January 4.

A Simple Heat Engine.

AT the last *soirée* of the Royal Society, a beautiful experiment was shown by Mr. Shelford Bidwell, illustrative of the fact that nickel ceases to be magnetic at a certain definite temperature. A piece of nickel was arranged as the bob of a pendulum. As long as the nickel was below a certain temperature it was held on one side by a magnet, but when it was heated by a spirit lamp-flame beyond a certain temperature it ceased to be attracted, and passed out of the range of the flame; it then cooled, and almost instantly returned to its first position, again to be released by the heat of the flame. It occurred to me that if a disk of nickel were placed in a certain position in a magnetic field, and then heated, it would continuously revolve. This on trial I found to be the case.

The experiment was arranged thus:—The nickel disk BC, 5 cm. in diameter and 1 mm. thick, was mounted on an axle passing through A; it was held in a frame so that the faces of the two poles, N, S, of an electro-magnet were at right angles to one another; the heat was applied at H; the disk revolved in the



direction shown. A great many different positions were tried, but the one indicated gave far the best results. The rotation began when the part of the disk above the flame reached 290° C. The distribution of the lines of force passing through the disk from edge to edge was examined by placing a sheet of a non-magnetic metal above the disk when cold and when heated, and sifting iron filings on to it. When cold, the lines of force were uniformly disposed across it; but when the disk was heated, only a few appeared to pass through the heated region. From this it is evident that the fall on the heated side of the disk is always less than that on the side which is not so hot, and to this the rotation is due. FREDERICK J. SMITH.

Trinity College, Oxford, December 31, 1891.

The Migration of the Lemming.

DR. ANDREW WILSON, in referring to my letter on the Norwegian lemming, is under the very natural misapprehension—which I formerly shared—that "only a miserable remnant of the original swarm" reach the sea. Now, although it is true that throughout their pilgrimage they are exposed to the attack of every rapacious bird, beast, and biped, and that even the Rendyr, which is by no means rapacious, never misses the opportunity of obtaining a *bonne bouche* of grass *à la saur krout* from their paunch, yet so prolific are the lemmings at this time that their numbers increase despite their enemies; some of which, be it remembered, do not dare to follow them when they leave the *fjeld*. During the last great migration I noticed that the little pilgrims became much more numerous as they ad-

vanced towards the *fjord*; but probably no single individual of those who began the exodus lives to share its fate—only the inherited impulse survives in the offspring. These animals may live in captivity for two years; mine, at least, did so; but, so far as I know, no one else has succeeded in keeping them nearly so long; and the reasons are curious. In the first place, they fight with each other incessantly, and irrespective of sex; and secondly, they invariably defile their supply of water, so that unless this can be made *running*, they are sure to perish.

I turned out my little colony on Richmond Green, at an hour when the almost ubiquitous boy was still abed, and I watched their behaviour with a box-compass and a butterfly net. The former article proved unnecessary, as they boxed the compass for themselves, and the latter inadequate, as they ate their way through the gauze with remarkable rapidity. I should add, however, that they were all eventually recaptured, and that I derived no information as to their sense of direction from the experiment. Dr. Wilson states that naturalists generally believe that the lemmings seek a "land of promise," or rather of past fulfilment. I was under the impression that the credit (or is it the reverse?) of the idea belonged to me, but under a sun which sees so little that is new, I may well be mistaken; yet, singly or jointly, rightly or erroneously, I still believe that these migrations were formerly of benefit to the species. That they are not so now, is obvious; but the chief interest seems to lie in their periodicity, the marvellous fecundity which supports them, and the remarkable faculty which directs them.

W. DUPPA-CROTCH.

Asgard, Richmond, January 14.

P.S.—Absence from home prevented me from noticing the letter of Prof. Romanes. To the former of his two queries I reply that all the migrations which I have noticed during twenty years have *crossed* my lake, which lies nearly north and south, whereas had they followed the valley and watershed they would have been spared this labour and risk. The same argument applies to Lake Mjosen and others. As regards the second query, whether I believe in a sub-tropical Atlantis or not seems to me to have as little bearing on a possible land-connection between Norway and Iceland as on the Goodwin Sands. It has been suggested to me that at the close of the latest glacial epoch the lemmings may have found it necessary to migrate to the warmer western shores of the peninsula: this, however, leaves the presence of the animals in Iceland unexplained, save by the rather vague action of flotsam and jetsam. In any case, I only wish to adopt the most convenient hypothesis, until it is disproved or supplemented by a better one.—W. D-C., January 18.

IN discussing the much-debated subject of the westward migration of the Norwegian lemming, the primary cause—as it appears to me—has been altogether overlooked.

This is, that the whole of Norway north of the Jotunhjem region—that is, the whole of the country of the Norwegian lemmings—is simply the steep and narrow westward slope of a long ridge of mountains.

When Mr. Collett says that "the wanderings take place *in the direction of the valleys*," he simply repeats in other words the usual description of their general westerly course.

They breed in the uplands, and when very prolific the increase must descend or perish, as they consume all the vegetation of their birth-region and no further supplies of food are obtainable either northward, southward, or eastward; but downwards, *i.e.* westward, the vegetation increases steadily as they proceed, and the descending autumn snow-line pushes onward behind them. Their devastation of meadows and oat-fields proves the urgency of their downward or westward course.

There are lemmings also on the eastern slopes of the Kjölen range, *i.e.* in Sweden. We are told that the Swedish lemmings proceed to the Gulf of Bothnia and are there drowned. To do this they must travel in the eastward and southward directions of a much longer slope than the steep westward course of the Norwegian lemming. A glance at a good map of Sweden and Norway will show all this.

W. MATTIEU WILLIAMS.

The Grange, Neasden.

The New Forest Bill, 1892.

IN connection with the petitions in favour of this Bill, to which the signatures of persons interested in the New Forest are being obtained, I am frequently asked, "What is the necessity for the Bill, and what is its object?" The facts of the case may be shortly stated as follows. The "Woods and Wastes" of the Forest comprise about 63,000 acres of land, the whole of which were, prior to 1698, open and uninclosed; but under the authority of the Acts 9 and 10 William III. c. 36 (1698) and 48 George III. c. 72 (1808), the Crown was empowered to inclose, and keep inclosed, freed and discharged from all rights of common, such quantity of land in the Forest as would amount to 6000 acres, for the growth of timber. By the Act of 14 and 15 Vict. c. 76 (the Deer Removal Act of 1851), the Crown was authorized to inclose and plant with trees any quantity of land, not exceeding 10,000 acres, in addition to the 6000 acres already in inclosure under the authority of the Acts before mentioned. The powers conferred by these Acts are not repealed by 40 and 41 Vict. c. 121 (the "New Forest Act, 1877"), but the rights of inclosure are by sec. 5 of the last cited Act limited to "such lands as are at the date of the passing of this Act inclosed, or as have, previously to such date, been inclosed by virtue of commissions issued in pursuance of the said Acts or some of them." The New Forest Act of 1877 practically secured the New Forest to the public, but the Act is virtually repealed by the 10th section of the Ranges Act, 1891 (and other Acts therein referred to), under the authority of which the War Department, with the consent of the Commissioners of Woods and Forests, can take possession of any part of the Forest for military purposes, and exclude the public from the enjoyment of any tract so taken. Already it is proposed to take 800 acres for a rifle range and the site of a camp, and there is nothing to prevent the exercise of such rights throughout the district, and the conversion of the Forest into a second Aldershot. Wherever a portion of the Forest is taken, the rights of the commoners, if they complain, will be bought up and extinguished; and thus, by taking different areas at different times, the Commissioners may before very long extinguish the common rights, and reduce the Forest into private ownership. It is clear that the proposed inclosure of 800 acres, and the user of the Forest generally in the way described, is in direct violation of the spirit and intention, as well as of the express provisions, of the New Forest Act of 1877.

The object, therefore, of the New Forest Bill is to make it clear that the Forest shall not be deemed to be within the provisions of the 10th section of the Ranges Act, 1891, and that the provisions of the New Forest Act, 1877, shall remain in force.

The rights secured by the Act of 1877 and the preservation of the Forest as an open space are of the greatest importance to naturalists, artists, and the general public, and every possible effort should be made to secure the passing of the Bill by signing petitions in support of it.

H. GOSS.

Entomological Society, 11 Chandos Street,
Cavendish Square, W., January 26.

A Brilliant Meteor.

LAST night, at 10h. 55m. G.M.T., I had the good fortune to witness the flight of a magnificently brilliant meteor. I was standing outside in the south-east re-entering corner of this building, and happened to be looking up at the constellation Leo, when the meteor suddenly flashed into sight from over the roof of the Observatory, a little east of the zenith, and not far from the stars κ and ι Ursæ Majoris, passed east of Procyon, and did not disappear till it had reached a position about 5° east of Sirius. An immediate reference to the map showed the positions of its appearance and disappearance to be about 9h. + 48° and 7h. − 15°.

For the greater part of its course it presented the appearance of a broad band of deep yellow light, but after it had passed about two-thirds of its path, it widened out into an elongated mass, distinctly rounded on the front, and of a full violet colour. From the middle of this round front the yellow band again emerged, and was finally lost to view about 15° or 20° further on. The violet mass would be about 5° in length. The whole apparition occupied 4 or 5 seconds, and the band of light was seen for an instant complete on the sky, stretching over some 65°.

About 10 minutes later a small meteor shot out from a point near the stars μ and λ Ursæ Majoris, and disappeared in the direction of Procyon.

THOMAS HEATH.

Royal Observatory, Edinburgh, January 25.

ON SOME POINTS IN ANCIENT EGYPTIAN ASTRONOMY.

I.

1. *Direction of Preliminary Inquiry.*

I HAVE recently been prosecuting some inquiries on the orientation of Egyptian temples which have led me to the conclusion that in all probability the temples, and the gods and goddesses in the Egyptian Pantheon to which they were dedicated, were in some way connected with the sun and certain stars. The method adopted in the research has been as follows:—

(1) To tabulate the orientations of some of the chief temples described by the French Commission, Lepsius, and others.

(2) To extend and check some of these observations with special reference to my new point of view, in Egypt.

(3) To determine the declinations to which the various amplitudes correspond. In this direction I have made use of the Berlin Catalogue of star places from 1800 A.D. to 2000 B.C.,[1] some places for Sirius and Canopus which have been obligingly placed at my disposal by Mr. Hind, and approximate values given by the use of a precessional globe constructed for me by Mr. Newton. This globe differs considerably from that previously contrived by M. Biot, about which I was ignorant when I began the work, and enables right ascensions and declinations, but especially the latter, to be determined with a fair amount of accuracy for twenty-four equidistant points occupied by the pole of the earth round the pole of the ecliptic (assumed to be fixed) in the precessional revolution.

(4) Having the declinations of the stars thus determined for certain epochs, I have next plotted them on curves, showing the amplitude for any year up to 5000 B.C. at Thebes for a true horizon and when the horizon is raised 1° or 2° by hills or mist.

(5) In cases where the date of the foundation of a temple dedicated to a particular divinity has been thoroughly known, there was no difficulty in finding the star the declination of which at the time would give the amplitude, and, in the case of series of temples dedicated to the same divinity, an additional check was afforded if the changes of amplitude from the latest to the newest temple agreed with the changes of the declinations of the same star.

This method also enabled me to suggest that certain temples, the date of foundation of which was not known, if they formed part of the same series, would thus have the date of original foundation determined.

(6) These results led me to the conclusion that certain stars had been used for temple purposes, to the exclusion of others.

(7) The next point, therefore, was to determine why these stars had been selected, and not others: and the precessional globe was used to study these stars, in relation to their heliacal rising and setting at different times of the year, but especially at the summer solstice.

(8) The *raison d'être* of the use of these stars at once became evident in a very remarkable fashion, and indicated that observations of them might certainly have been made to herald sunrise.

In some cases the star rose heliacally with the sun, or thereabouts; in others, it set heliacally—that is, when the sun was 10° below the eastern horizon.

2. *The Building Ceremonials recorded in the Inscriptions.*

In a paper presented to the Society of Antiquaries in May last, and elsewhere, I have given reasons to show that the temple of Amen-Rā at Karnak was built in such a manner that at sunset at the summer solstice—that is, on the longest day in the year—the sunlight entered the temple and penetrated along the axis (more than a quarter of a mile in length) to the sanctuary. I also pointed out that a temple oriented in this manner truly to a solstice was a scientific instrument of very high precision, as by it the length of the year could be determined with the greatest possible accuracy provided only that the observations were continued through a sufficient period of time.

All the temples in Egypt, however, are not oriented in such a way that the sunlight can enter them at this or any other time of the year. They are not therefore solar temples, and they have not this use. The critical amplitude for a temple built at Thebes so that sunlight can enter it at sunrise or sunset is about 26° north and south of east and west, so that any temples facing more northly or southly are precluded from having the sunlight enter them at any time in the year.

Thus at Karnak, to take an instance, there are two well-marked series of temples which cannot, for the reason given, be solar, since one series faces a few degrees from the north, and the other a few degrees from the south. There are similar temples scattered all along the Nile Valley. The first question, then, to ask of the inscriptions is if there are records that these temples were directed to stars, as the solar temples were to the sun?

As a matter of fact numerous references to the ceremonial of laying the foundation-stones of temples exist, and we learn from the works of Chabas, Brugsch, Dümichen,[1] and others, that the foundation of an Egyptian temple was associated with a series of ceremonies which are repeatedly described with a minuteness which, as Nissen has pointed out,[2] is painfully wanting in the case of Greece and Rome. Amongst these ceremonies, one especially refers to the fixing of the temple-axis; it is called, technically, "the stretching of the cord," and is not only illustrated by inscriptions on the walls of the temples of Karnak, Denderah, and Edfu—to mention the best-known cases—but is referred to elsewhere.

Another part of the ceremony consisted in the king proceeding to the site where the temple was to be built, accompanied mythically by the goddess Sesheta, who is styled "the mistress of the laying of the foundation-stone"

Each was armed with a stake. The two stakes were connected by a cord. Next the cord was aligned towards the sun or star as the case might be; when the alignment was perfect the two stakes were driven into the ground by means of a wooden mallet; there was no difference of procedure in the case of temples directed to the sun. One boundary wall parallel to the main axis of the temple was built along the line marked out by this stretched cord.

If the moment of sun- or star-rise or set were chosen, as we have every reason to believe was the case, seeing that all the early observations were made on the horizon, it is obvious that the light from the body towards which the temple was thus aligned would penetrate the axis of the temple thus built from one end to the other in the original direction of the cord.

We learn from Chabas that the Egyptian word which expresses the idea of founding or laying the foundation stone of a temple is *Senti*—a word which still exists in Coptic. But in the old language another word *Put-ser*, which no longer remains in Coptic, has been traced It has been established that *Put* means to stretch, and *Ser* means cord, so that that part of the ceremonial which consisted in stretching a cord in the direction of a star was considered of so great an importance that it gave its name to the whole ceremonial.

I will next refer to some of the inscriptions; one, dating from the last half of the third thousand B.C., occurs in

[1] *Vierteljahrsschr. der Astronomischen Gesellschaft*, vol. xvi. p. 9, 1881.

[1] "Baugeschichte des Dendera-Tempels." 1877.
[2] *Rheinisches Museum für Philologie*, 1885, p. 39.

the document describing the building of the temple of On (Heliopolis). We read :—"Arose the king, attired in his necklace and the feather crown; all the world followed him, and the majesty of Amenemha [first king of the XIIth dynasty]. The Kolchyt read the sacred text during the stretching of the measuring-cord and the laying of the foundation-stone on the piece of ground selected for this temple. Then withdrew His Majesty Amenemha; and King Usertesen [son and co-regent] wrote it down before the people."

Nissen, from whom (*loc. cit.*) I quote the above, adds :—"On account of the stretching of the measuring cord, the Egyptian engineers were called by the Greeks ἁρπεδονάπται, whose art Democritus boasts of having acquired."

We next turn to Abydos, possibly one of the oldest temple-fields in Egypt. There is an inscription relating to the rebuilding of one of them in the time of Seti I. (about 1445 B.C.). In this the goddess Sesheta addresses the king as follows :—"The hammer in my hand was of gold, as I struck the peg with it, and thou wast with

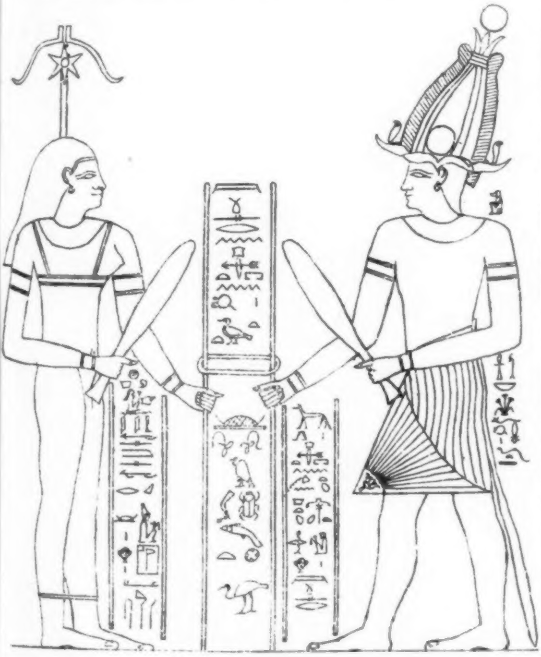


FIG 1.—The king and the goddess Sesheta stretching the cord at the foundation of the Temple of Denderah. (From Dümichen.)

me in thy capacity of Harpedonapt. Thy hand held the spade during the fixing of its [the temple's] four corners with accuracy by the four supports of heaven." On the pictures the king appears with the Osiris crown opposite the goddess. Both hold in their right hand a club, and with it they each hammer a long peg into the ground. Round the two pegs runs a rope, tied together at the ends, which is stretched tight.

In two cases the star used for the alignment is actually named. Of these I will take, first, the record of the ceremony used in the building of the temple of Hathor at Denderah.

3. *The Alignment of the Temples of Denderah and Edfu.*

Denderah.—The inscriptions state that the king while stretching the cord had his glance directed to the *ak* of the constellation of the *Thigh*—the old name of the constellation which we now recognize as the Great Bear, and on this line was built the new temple, "as had been done there before."

The actual inscription has been translated as follows :—"The living God, the magnificent son of Asti [a name of Thoth], nourished by the sublime goddess in the temple, the sovereign of the country, stretches the rope in joy. With his glance towards the *ak* [the middle?] of the Bull's Thigh constellation, he establishes the temple-house of the mistress of Denderah, as took place there before." At another place the king says: "Looking to the sky at the course of the rising stars, [and] recognizing the *ak* of the Bull's Thigh constellation, I establish the corners of the temple of Her Majesty."

Here, then, we have more than evidence of the stretching of a cord towards a star; an actual constellation is named, and it may be easily imagined in connection with this that many interesting questions arise of the utmost importance to the subject we are considering.

Dümichen, in his references to this passage, discusses

FIG. 2.—The constellations of the Hippopotamus and Thigh, from the centre of the Zodiac of Denderah.

the meaning of the word *ak* in relation to some Theban grave inscriptions, in which it is suggested that *ak* is used to represent the middle course of a star, or, astronomically speaking, its culminating point as it passes the meridian. But such a meaning as this will never do in this connection; for if a cord was stretched towards a star on the meridian it would lie north and south, and therefore the temple would be built north and south. But this is by no means the orientation of the temple—a point to which I shall return presently.

But it may be suggested that the word *ak*, used in relation to the king's observation, more probably referred to the "middle point" of the constellation which would be about represented by the star α, which lies nearly in the centre of the modern constellation of the Great Bear, supposing, indeed, that the same stars were included in the old constellation; but on this point we certainly have

no definite knowledge, as the Thigh is so variously represented; sometimes there is a hind-quarter, represented evidently by the well-known seven stars; at others, the body of a cow (with horns and disk) is attached.

However this may be, without such a reference to some particular part of the constellation it is obvious that the stretched cord must have had a most indeterminate direction.

In order to leave no stone unturned in attempting to explain this description—supposing it to represent an undoubted fact of observation, there is another possible interpretation of the word *ak* which we may consider. The amplitude of the temple being 73° N. of E., shows conclusively that we cannot be dealing with the meridian, but may we be dealing with the most eastern elongation of the star in its journey round the Pole?

I have inquired into this matter for the time of the last building of the temple in the time of the Ptolemies, and find that the amplitude of the temple, instead of being 73°, would have been about 70°. It seems probable, then, that this interpretation will not hold, and it may be further stated that, in the case of a star at a considerable distance above the horizon, the stretching of a cord in the building ceremonial – the "ausspannung der strickes," as the words *put-ser* are translated by Dümichen—would really have been no stretching of the cord at all, for the star being many degrees above the horizon, another

the wooden peg and the handle of the club; I hold the rope with Sesheta; my glance follows the course of the stars; my eye is on Masχet [that is, the 'Bull's Thigh constellation,' or Great Bear]; (mine is the part of time of the number of the hour-clock); I establish the corners of thy house of God." And in another place: "I have grasped the wooden peg; I hold, the handle of the club; I grasp the cord with Sesheta; I cast my face towards the course of the rising constellations; I let my glance enter the constellation of the Great Bear (the part of my time stands in the place of his hour-clock); I establish the four corners of thy temple." The translation is Brugsch's. The phrases in brackets are interpreted differently by Dümichen, who translates them: "Standing as divider of time by his measuring instrument," or "representing the divider of time (*i.e.* the god Thot) at his measuring instrument." The word *merech* or *merchet*, in which Brugsch suspects hour- or water-clock, does not occur elsewhere.

In this case, seeing that the temple lies with its axis very nearly north and south, as I determined by my own (magnetic) observations, the stretching of the cord was certainly in or very near the meridian; and it may be remarked that in the naos there is an opening in the roof, over the side of the second or third door from the sanctuary, and inclined at an angle of 40° (unlike any other opening that I have seen in the roof of any

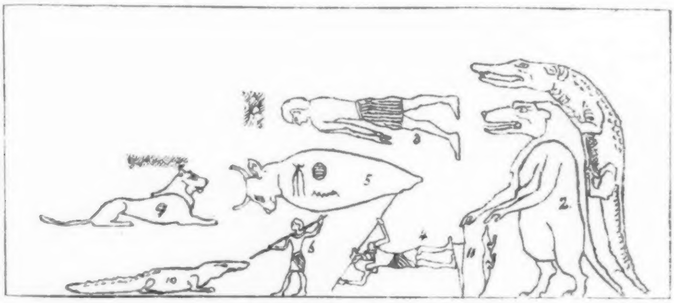


FIG. 3.—Another form of the constellation of the Thigh. (From Brugsch.)

method must have been employed, and in all probability would have been distinctly referred to in the careful statements of the ceremonies which exist.

I think, then, that we are perhaps justified in discussing this possible explanation, especially as rising stars are referred to. We now come to considerations of a different order. The inscription which we have quoted is put into the mouth of the Emperor Augustus, though he never was at Denderah.

This suggests that the temple built in the time of Augustus carried forward the account of the old foundation. There is evidence of this. The constellation of the Thigh neither rose nor set in the time of Augustus—it was circumpolar. The same statement may be made regarding the restoration in the time of Thotmes III. So we are driven to the conclusion that if we regard the inscription as true, it must refer to a time preceding the reign of Thotmes.

Edfu.—A reference to the same constellation (the Thigh) is also made in the account of the ceremonial used at the laying of the foundation-stone of the temple at Edfu. The king's glance was directed—in the case of the building of that temple—to the *Thigh*, but no precise reference to any star or to any point *ak* is given.

As before, I give the full translation of the inscription,[1] remarking that the last restoration was made B.C. 237–57. The king is represented as speaking thus: "I have grasped

1 Quoted from Nissen.

Egyptian temple), which may have been used to observe the transit of some particular star. The angle I was not able to determine with absolute accuracy, as the vertical circle of the theodolite I had with me was out of adjustment.

Taking the latitude of Edfu as 25°, and assuming the angle of 40° to be not far from the truth, the North Polar distance of the star observed would be 15°.

This satisfies within a degree or so—and this is as near as we can get till more accurate observations have been made on the spot—Dubhe, the chief star in the Great Bear in the time of the Ptolemies.

I may here remark that, so far as I know, Edfu is the only temple in Egypt on the meridian. If, therefore, it were used, as on my theory all other temples were, it could only have picked up the light from each of the southerly stars, as by the precessional movements they were brought into visibility very near the southern horizon.

In this respect, then, it is truly a temple of Horus, in relation to the southern stars—the southern eyes of Horus. But it was not a sun-temple in the sense that Karnak was one; and if ceremonies were performed for which light was required, perhaps the apparatus referred to by Dupuis (vol. i. p. 450) was utilized. He mentions that in a temple at Heliopolis, whether a solar temple or not is not stated, the temple was flooded all day long with sunlight by means of a mirror. I do not know the

authorities on which Dupuis founds his statement, but I have no doubt that it is amply justified, for the reason that doubtless all the inscriptions in the deepest tombs were made by means of reflected sunlight, for in all freshly-opened tombs there are no traces whatever of any kind of combustion having taken place even in the innermost recesses. So strikingly evident is this that my friend M. Bouriant, while we were discussing this matter at Thebes, laughingly suggested the possibility that the electric light was known to the ancient Egyptians.

With a system of fixed mirrors inside the galleries, whatever their length, and a movable mirror outside to follow the course of an Egyptian sun and reflect its beams inside, it would be possible to keep up a constant illumination in any part of the galleries, however remote.

Dupuis quotes another statement that the greatest precautions were taken that the first rays of sunlight should enter a temple (of course he means a solar temple).

J. NORMAN LOCKYER.

(*To be continued.*)

ON THE NUMBER OF DUST PARTICLES IN THE ATMOSPHERE OF VARIOUS PLACES IN GREAT BRITAIN AND ON THE CONTINENT, WITH REMARKS ON THE RELATION BETWEEN THE AMOUNT OF DUST AND METEOROLOGICAL PHENOMENA.—Part II.[1]

THIS paper contains the results of the observations made on the dust of the atmosphere at various places in 1890. These observations were made by the author at the same stations, and about the same dates, as those made in 1889, and given in Part I. of this subject, read before the Society on February 3, 1890.

At Hyères, in 1890, the highest number of dust particles observed was 15,000 per c.c., with a wet bulb depression of 5°, the atmosphere at the time being very thick. The lowest was 725 per c.c., with a wet bulb depression of 9°·5, when the air was very clear.

At Cannes very few observations were made on this occasion. The numbers varied from 1275 to 2850 per c.c. The wind during the time was always northerly.

At Mentone the numbers varied from 26,000 per c.c. when the wind was from the town, to about 900 per c.c. when it was from the mountains, with a wet bulb depression on both occasions of 3½°; the air was clear with the lower number and thick with the higher.

At Bellagio, when the wind was southerly—that is, from the inhabited districts—the number of particles was great, on one occasion as high as 20,000 per c.c. But when the wind blew from the north—that is, from the direction of the Alps—the number fell as low as 600 per c.c. on one occasion. With the low numbers the air was clear, whereas with the high numbers there was always a good deal of haze, though the air was dry.

All the observations made at Baveno were made while the wind blew from the inhabited areas, and the air was never clear, although on some days it was very dry. The highest number observed at this station was 16,000 per c.c., and the lowest 2000 per c.c.

The observations made at the Rigi Kulm from May 15 to 20 are then discussed. There was a marked difference in the appearance of the air on this occasion compared with what was seen on the first visit. During the previous visit the weather was generally fine, and the air had that crisp clearness which gives the hard outline and crude colouring one generally associates with Swiss scenery; whereas on the second visit the air was remarkably thick and heavy.

[1] Abstract of a Paper read before the Royal Society of Edinburgh on January 4, by John Aitken, F.R.S. Communicated by permission of the Council of the Society.

The highest number observed on the first visit to the Rigi was not much over 2000 per c.c., while the number was as high as 10,000 on the second. The same relative condition of impurity existed at the low level also. On the first occasion the number at the level of the lake varied from 600 to 3000 per c.c., whereas on the second visit they varied from 1700 to 13,000 per c.c. Roughly speaking, there was about four times the amount of dust in 1890 there was in 1889, and the air was about four times as thick.

On the way up the Rigi the air was tested at the level of the lake and found to have about 11,000 particles per c.c. There was a very thick haze at the time, through which the mountains loomed darkly. This thickness was evidently not due to humidity, as the wet bulb showed a depression of 10 degrees. This very thick haze was therefore due to fairly dry particles of dust. On arriving at the top of the mountain in the afternoon, the air was tested and found to have slightly over 4000 particles per c.c.

During the first four days of the second visit to the Rigi Kulm the air was very thick and the number of particles great. The much greater thickness of the air on the occasion of the second visit was evidently due to dust, as the humidity on both occasions was about the same. The air on the days when the number of particles was great was very different from anything previously observed at this station. On the first visit the air was clear and bright, with only a thin haze between the observer and the distant mountains; whereas in 1890 a dense haze hung in the atmosphere, so thick that towards sunset the lower slopes of Pilatus could scarcely be distinguished. It looked as if a veil had been hung up between the observer and the distant scenery. Some time before sunset this hazy veil became coloured by the rays of the setting sun. Its upper limit was well defined in the eastern sky, at an elevation considerably above the highest of the Alps. At sunset this dusty impurity became still more apparent as the earth's shadow crept up its lower edge. Though the sky was cloudless, so dull was the setting sun, that it looked more like a harvest moon than the orb of day. So feeble were its rays after penetrating the thick haze, that they could produce no direct red light on the mountains, while much diffused light was reflected by the dust-laden air.

During this second visit there was an opportunity of testing the supposed influence of thunderstorms in depositing the dust in the atmosphere. On one of the days of this visit a violent thunderstorm raged to the east, south, and west during most of the afternoon, and in the evening it came over the Rigi Kulm. So near was the storm that the flash and crash of the thunder seemed simultaneous. The tests were therefore made in the very air in which the lightning discharges were taking place. During the day, and before the storm approached, there were nearly 4000 particles per c.c. in the air. At 6 p.m., when the storm was near, the number fell to 3000; and at 7.10 p.m., when the storm was nearly over, the number was as low as 725 per c.c. These figures seem to support the supposition that thunderstorms purify the air; and if anyone who was a believer in the purifying influence of these storms had been on the top of the mountain next day, his opinion would have been confirmed by the greatly improved appearance of the atmosphere after the storm. The thick veil which had hung in the atmosphere for at least four days was gone, and the distant mountains looked clear and distinct. Even Hochgerrach, which is about 70 miles distant, was quite distinctly seen during the whole day; and the number of particles fell to 400 per c.c.

The question—Was the decrease in the dust, and the improved appearance of the atmosphere on this occasion due to the thunderstorm?—is then discussed. It is shown that this conclusion is extremely doubtful. It is

pointed out that the violent down-rush of air produced by a heavy hail-shower at the time would bring down the purer upper air to the place of observation, so that the air tested at 7.10 p.m. was not the same as that tested previously, but was air from a higher and purer stratum. The purifying influence of the down-rush of air in this case was not nearly so great as was observed in the heavy rainfall on the Eiffel Tower recorded in Part I.

By midday of the last day of the visit to the Rigi Kulm, the air again became very much hazed, and the number of particles rose to about 10,000 per c c. On descending the mountain, the air was again tested on this day, at the level of the lake, at 3 p.m. Here the number was a little over 10,000, or very much the same as it was when tested on the way up. Its humidity was also the same, and it had the same thick appearance.

Just when on the point of finishing the tests at the level of the lake, it was observed that the numbers were becoming unsteady and were falling. The tests were therefore continued for a considerable time longer, when it was found that the dust particles, which at first were 10,250 per c.c., gradually decreased to 1700 per c.c.; a most unusual experience, and one which might have shaken our confidence in the dust-counter, had it not been noticed that this decrease in the dust was at the same time accompanied by a rise in the temperature of the air, and by a decrease in its humidity. When the tests were begun, and the number of particles was great, the temperature was 71°, and the wet bulb was depressed 11 degrees; but when the amount of dust was small, the temperature had risen to 74°·5, and the wet bulb depression was as much as 18·5 degrees. These observations all show that in a very short time there had been an entire change of the air at the place of observation. This result is shown to have been produced by the local wind, which at first blew in from the lake, changing by the upper part of the current striking the nearly perpendicular face of the mountain, curving downwards, and then blowing out towards the lake, thus bringing a purer upper air to the place of observation. After a time the down-blow ceased, and the wind at the level of the lake returned to its original direction. After the wind had blown a short time off the lake, the number of particles rapidly increased, and became slightly higher than it was at first. The temperature and humidity also returned to near their original readings.

The Rigi Kulm observations show the daily maximum of dust very clearly. On all the days, except one, the number of particles was least in the morning, and increased greatly as the day advanced, owing to the ascent of valley air on the sun-heated slopes of the mountain. The impure air had generally arrived at the mountain top before midday, and by midday the number was generally three times greater than it was in the morning.

Observations were also made on one day on Pilatus Kulm. During the whole day the mountain was covered with cloud. On this occasion the numbers were found to be very unsteady, varying greatly in short intervals. The highest reading was 1275 per c.c., and lowest 625 per c.c.

The result of an investigation into the cause of the difference observed in the air of Switzerland on the two occasions is then given. It is shown that during the days of the first visit the upper air circulation was generally from the south, and was pure, as it came from the uninhabited area of the Alps; while during the first days of the second visit the general air circulation was northerly—that is, from densely inhabited and polluted areas. While this northerly circulation continued, the air was thickly hazed, and the number of particles was great. On the morning of the day of the thunderstorm already referred to, the wind had changed and begun to blow freshly from the south, at the St. Gothard. It had also changed to south on the Säntis and Rigi. But though the wind had begun to blow from a pure direction in the morning, it was evening before the pure air arrived at the Rigi. It was at the meeting area of the lower impure northerly air with the upper pure southerly that the thunderstorm took place. The storm began to the south, where the currents first met, and travelled northwards as the pure south wind drove the northerly air before it. The sudden drop in the number of dust particles in the storm may therefore possibly have been due to the arrival of the pure southerly air..

The day after the storm the southerly wind continued to blow, the air became very clear, and the number of particles fell practically as low as was observed on the previous visit, while the air had much the same clear appearance it had on that occasion. It would thus seem that the clearing of the atmosphere on this occasion, though at first sight it may have appeared to have been caused by the thunderstorm, was in reality caused by a change in the circulation of the air. The clearing would thus appear to have been due, not to a clearing of the dust out of the air, but to a change of the air itself.

The observations made on Ben Nevis and at Kingairloch, for July 1890, are then discussed, and a comparison made of the variations in the amount of dust at high and low levels. The observations made at Kingairloch in 1890 confirm the conclusions arrived at in Part I., that the air at this station has most dust in it when the wind blows from the east, south-east, and south—that is, from inhabited areas—and less when the wind blows northerly—that is, from uninhabited areas. Some very remarkable exceptions to the latter conclusion were, however, observed. On a number of days when the wind was northerly the number of particles rose high at some hour of the day. On examining into the cause of the exceptional readings with northerly winds, it was found that they almost always occurred when the isobars over our area were irregular and the general circulation in a confused condition, and blowing in different directions at different stations at no great distances from each other. It is suggested that under these conditions uniformity in the air cannot be expected; that, while testing in a northerly wind, it may be southerly air that is being examined. In confirmation of this it is shown that whenever the general air circulation over our area was mixed and irregular, high numbers were also observed at some time of the day on Ben Nevis.

An examination of the numbers of dust particles shows that there was much less dust in 1890 than in 1889. The numbers in 1890 fell very low on many days, and extremely low on a few days. The lowest number observed was 16·5 per c.c. This number is much lower than any previously observed at any low-level station. Associated with the small amount of dust was an exceptionally low temperature. July of 1890 will long be remembered as one of the most inclement experienced for many years, being cold, wet, and windy.

A comparison is made of the amount of dust at Kingairloch and Ben Nevis. Although there is a considerable resemblance between the figures at the two stations, yet the likeness is not very close. This is owing to the daily maximum of dust which takes place at high levels on most days; also to the effect of the wind on the amount of dust not being the same at high and low levels; and further, the directions of the winds are not always the same at both stations. As a rule, there was much less dust at the high station than at the low one, and when the dust increased at the low station it also generally increased at the high one, and *vice versâ*. These rules, however, are not without exceptions. Sometimes the lower air was purer than the upper; this happened when the wind blew from a pure direction low down, while it was from the east or south on the Ben. Mr. Rankin has shown, from an extensive series of observations at the Observatory, that the south, south-east, and east winds bring the most impure air to

the Ben—a conclusion in keeping with the result obtained at low level.

It is concluded from the observations that high winds reduce the transparency of the air. In Part I. this conclusion is indicated, and the observations of 1890 confirm it. It is pointed out that whenever the wind was high, the air was unduly thick for the number of particles and the humidity. This is thought to be due to high winds carrying large particles, and mixing the lower stratum of impure air with the purer upper air. The inequalities in the density of the different parts of the air produced by imperfect mixing will also reduce its transparency.

The Alford observations for 1890 show that the air was occasionally purer and the maximum a little higher in that year than on the previous visit. Whenever the wind blew from the south, it brought polluted air to this station, as it came from inhabited areas; and when the wind was northerly the air was pure. The number of particles was as low as 127 per c.c. with a north-west wind, while it was as high as 6800 per c.c. with a south wind.

An ascent of Callievar was made in 1890 also. On the first visit the air was clear, and the Cairngorms and Lochnagar were clearly seen. The number of particles was 262 per c.c., and rose in the afternoon to 475 per c.c.; but on the second visit the air was thick, and only a faint outline of the Cairngorms was occasionally seen, while Lochnagar was quite invisible. The number of particles was 710, and rose in the afternoon to 1575 per c.c.

The air on this occasion was very irregularly hazed, not being equally transparent in all directions. One mass of air darkened the view to the west, passed over the hill-top and darkened the view to the east. Before this impure mass of air arrived at the hill-top the number of particles was 710, while it was passing the number rose to 1575, and after it had gone east the number fell to 1050 per c.c. During these observations the humidity remained constant. The variations in the transparency were therefore due to variations in the amount of dust.

The condition of the air during the exceptionally warm February of 1890 was tested at Garelochhead on the 27th of the month. Previous to that date the weather had been very warm, temperatures of 50° and 60° having been frequently recorded in our area, and even 64° was observed in more places than one. The result of the tests showed the air to be remarkably full of dust. During the visit to this station in the end of January 1889, the maximum number of particles observed was 2360, and that was the only occasion on which it was over 1000; whilst on the first day of the second visit the smallest number observed was 7250, and other readings gave nearly 10,000. During this warm period the air was always impure, and had much the same appearance as it had on the 27th. The cause of this great amount of impurity was the presence of an anticyclone lying over Europe, giving rise to southerly winds over our area. The local winds were, however, very light and mixed, and there was no general circulation of the air; the dust impurities therefore accumulated, and, as the figures show, became very great. On the 28th, the day after the air was tested, a depression appeared off the north of our islands, and the isobars were closing in and westerly winds were beginning to blow. With this change the dust began to fall, and was as low as 1750 per c.c. on the 28th. On March 1 and 2 the isobars closed in still further, the winds freshened, and the dust fell to 51 per c.c., or $\frac{1}{200}$ of what it was on February 27. During the 3rd, 4th, and 5th, the wind remained in the north-west, and the amount of dust was very small.

Certain relations between isobars and dust are pointed out. With regular isobars for westerly and northerly winds the air is pure, and the closer the isobars the purer is the air; whilst isobars for southerly or easterly winds, even though close, do not indicate pure air. From these facts it is shown that an estimate of the amount of dust on any day can be made from an examination of the weather charts made on and previous to the day selected.

The relation between the amount of dust and the temperature is discussed, with the view of finding whether the observations made in 1890 confirm the conclusion arrived at from the previous records. That conclusion was that a great amount of dust increases the day temperature and checks the fall of temperature at night. The records of temperature and radiation made at Kingairloch in 1890 are of no value, owing to the weather being always under the influence of cyclones, so that there was an absence of clear skies, and the temperatures were regulated by what the winds brought, and were but little influenced by local conditions. But, as already stated, the dust at this station was exceedingly low in 1890, and the temperature was also exceptionally low.

The Alford observations, however, are not open to the same defect, as the weather was suitable for the purpose. These observations point to the same conclusion as that arrived at in 1889. The highest maximum temperatures were recorded on days of high dust, and the lowest minimum when the dust was at a minimum.

The observations made at Garelochhead also support the same conclusion. Towards the end of February the amount of dust was great, and from the meteorological report it will be seen that the temperature was above the mean, and was frequently very high. Again, when the westerly winds swept away the great impurity, they brought with them a high mean temperature. But after the winds ceased to blow, the pure air brought to our area by them seems to have allowed radiation to act freely, as the air then rapidly cooled, and the temperature became exceedingly low, as much as from 8 to 11 degrees below the mean in some stations in Scotland. The Ben Nevis observations show that during this exceptionally cold period the air was remarkably free from dust.

JOHN COUCH ADAMS.

IT is with deep regret that we record the death of Prof. Adams, who will always hold an eminent place in the history of astronomical science. As he is included in the list of our "Scientific Worthies," we have already given an account of his career (vol. xxxiv. p. 565). It is only necessary for us now, therefore—as in the case of Sir George Airy—to note some of the leading facts of his life and work.

He was born at Lidcot, near Launceston, in Cornwall, on June 5, 1819. He received his early education at the village school and at Devonport, where he gave evidence of his remarkable faculty for mathematical and astronomical study. In October 1839, he entered at St. John's College, Cambridge; and in 1843 he graduated as Senior Wrangler and first Smith's Prizeman, becoming shortly afterwards a Fellow and tutor of his College.

Both before and after taking his degree he was fascinated by a problem which was at that time profoundly interesting astronomers—the irregularities shown by the planet Uranus in its motion. Its orbit differed from the elliptic path which an undisturbed planet would have pursued; and as the deviations could not be explained by the influence of the other known planets, it was supposed that there must be a more remote planet which had not then been observed. To the search for this unknown planet Adams devoted all the energies of his mathematical genius, and everyone knows the brilliant success with which his labours were crowned. His solution was communicated to Prof. Challis in September 1845, and to the Astronomer-Royal in the following month. We need only refer to the facts that similar work was done in 1846 by Leverrier; that the French astronomer's results, unlike those of the English investi-

gator, were at once made known; and that on September 23, 1846, the planet Neptune was found by Dr. Galle, of Berlin, on the basis of Leverrier's elements. Adams and Leverrier rank as joint discoverers, and, as such, they received on February 11, 1848, the gold medal of the Royal Astronomical Society. Some members of Adams's college, in order to mark their sense of the importance of his achievement, raised a fund, which the University accepted, for the founding of a prize, to be called "The Adams Prize," to be awarded every two years to the author of the best essay on some subject of pure mathematics, astronomy, or other branch of natural philosophy. In 1851 he was elected President of the Royal Astronomical Society.

As he did not take orders, his Fellowship at St. John's expired in 1852, but he continued to reside in the College until 1853, when he was elected to Pembroke. In 1858 he was appointed Professor of Mathematics at the University of St. Andrews, but he held this office only during a single session. He became the Lowndean Professor of Astronomy and Geometry, at Cambridge, in 1859, in succession to the late Prof. Peacock, and retained this position during the remainder of his life.

Meanwhile, he had been carrying on many important investigations; and, until ill-health disabled him, his labours were never seriously interrupted. Foremost among his later achievements were the results of his researches on the moon and on the theory of the November meteors. In 1866 the Royal Astronomical Society awarded him its gold medal for his lunar researches. He had succeeded Prof. Challis as Director of the Cambridge Observatory in 1861, and in 1884 he served as one of the delegates for Great Britain at the International Meridian Conference at Washington.

For about a year and a half before his death, Prof. Adams was too ill to do as much work as he had been accustomed to do, and during the last ten weeks he was confined to bed. He died on the morning of January 21.

He was a Fellow of the Royal Society, and of the leading foreign scientific bodies; and honorary degrees were conferred upon him by his own University and by Oxford. The post of Astronomer-Royal was offered to him by the First Lord of the Admiralty in 1881, on Sir George Airy's retirement, but declined by him on the ground of age.

WALTER HOOD FITCH.

THIS talented botanical artist, whose name appears in almost every illustrated work of importance on botany or horticulture that was published in this country during the half-century from 1835 to 1885, expired at his residence at Kew on the 14th inst., after several years' indisposition, in which mental and physical decay were combined. The deceased was 75 years of age, and his whole life from early youth had been devoted to botanical drawing and painting; and his reputation was so high and so world-wide that it is unnecessary to say much on this point. Nevertheless, some particulars of the work of a man who accomplished so much and so well may be interesting to many persons who only know his work. Of Scotch birth, he was apprenticed, while still very young, to the designing department in a manufactory of fancy cotton goods at Paisley. Here his natural aptitude for drawing developed so rapidly and to such a degree as to indicate that he possessed talents of no ordinary kind, and his name soon became known outside of the factory. By some means he came under the notice of a friend of the late Sir William Hooker, and he, knowing that the latter was in need of a draughtsman, strongly recommended him to try the youth's capabilities. Sir William Hooker, at that time Regius Professor of Botany at Glasgow, acted on this suggestion, and the result was so satisfactory that he negotiated the cancel of Fitch's indentures, took him into his sole employ, and trained him for the kind of work he wished him to execute. We have not ascertained the exact date of this event, but it must have been as early as the year 1832, for already in 1834 he was a contributor to the *Botanical Magazine*, and he continued his connection with this long-lived periodical down to 1878, having during this period drawn and lithographed some 3000 of the plates. At first his initials did not appear regularly on the plates, but, on reference to the volume for 1837, it may be seen that it was practically all his, and that he had already become an efficient botanical draughtsman. The same year (1837) the first volume of Hooker's "Icones Plantarum" was published, and although Fitch's name does not appear, we have other evidence that he was the artist. In short, he not only illustrated all the numerous works of his first patron, but also those of his son, now Sir Joseph Hooker, as well as those of numerous other public and private persons. The fertility of his pencil was equalled by its facility, grace, vigour, and boldness; and his colouring was usually rich, and full, and truthful. It is true that most of his work does not exhibit the finish and minute detail characteristic of the masterpieces of the productions of the few other botanical artists with which comparisons could be made. In 1841, Sir William Hooker was appointed Director of the Royal Gardens, Kew, Fitch accompanying him, and residing there until his death. At Kew he found full scope for his powers, and notable amongst the numerous productions of his best days are the magnificent elephant folio plates representing various stages of the development of the *Victoria regia* as cultivated at Kew and Syon House; the plates of Sir William Hooker's numerous works on ferns; of Sir Joseph Hooker's "Botany of Sir James Ross's Antarctic Voyage"; and his "Illustrations of Himalayan Plants and Himalayan Rhododendrons"; of Howard's "Quinologia": of Bateman's "Odontoglossum"; of Welwitch's "West African Plants"; of Speke and Grant's "Plants of the Upper Nile"; and of Seemann's "Botany of the Voyage of the *Herald*." Examples of his later work are to be found in Elwes's "Lilies," and the botany of Salvin and Godman's "Biologia Centrali-Americana," the latter the last important work he accomplished. As might be imagined from the amount of work he did, Fitch wielded the pencil with remarkable rapidity and freedom; and one could not but admire the way in which he stood up and, free handed, guided his pencil over the stone without any preliminary drawing. Botanical drawing, however, is not a very lucrative profession, and therefore not likely to attract persons of great attainments; but when Fitch became incapacitated through failing health, his merits were so far recognized as to gain him a Civil List pension, on the recommendation of the Earl of Beaconsfield, of £100 a year.

NOTES.

MARCH 17 is the date fixed for the Bakerian Lecture of the Royal Society, and Prof. James Thomson is to be invited to deliver it. The Croonian Lecture is to be delivered on March 24 by Prof. Angelo Mosso, of Turin, the subject being "The Temperature of the Brain."

AT the Council Meeting of the Royal Society on the 21st inst., no fewer than ten deaths were announced, seven of the deceased having been Fellows of the Royal Society, and three Foreign Members. Taking into account that the average number of deaths for the whole year is fifteen, such a list for a single month is quite extraordinary.

PHYSIOLOGICAL science has sustained a severe loss by the death of Dr. Ernst von Brücke, the well-known Professor of Physiology at the University of Vienna. He died at Vienna on January 8, in his seventy-third year. He was a pupil of Johannes Müller, and made many contributions of first-rate importance to the study of physiology.

WE regret to have to record the death of Mr. Thomas Roberts, F.G.S., of St. John's College, assistant to the Woodwardian Professor of Geology at Cambridge. He died on Saturday last at the age of thirty-five. Mr. Roberts obtained a first class in the Natural Sciences Tripos, Part I., in June 1882, and in the second part of the same Tripos for geology in June 1883. He also won the Sedgwick Prize for a geological essay in 1886. Prof. Hughes, in his annual reports to the Senate, often alluded to the value of the services rendered by Mr. Roberts to the students of his classes.

THE Sydney papers announce the death of Sir William Macleay, who did much to promote an interest in science in New South Wales. It was mainly through his efforts that the Linnean Society of New South Wales was founded; and on many occasions he acted towards it with splendid liberality. The building in which it meets was erected at Sir William's expense. This building he transferred to the Society with the lease of the land on which it stands, giving at the same time, by way of endowment, a mortgage of £14,000 bearing interest at the rate of 5 per cent. per annum. He provided an excellent reference library, and equipped the rooms with fittings, furniture, and apparatus for scientific research. According to a speech delivered by one of the presidents, and quoted by the *Sydney Morning Herald*, he also bore the greater part of the expenses of the Society's publications, supplied the salaries of its officers, and "furnished its specialists with abundant funds for their investigations and their maintenance." Besides, he was the chief instrument in obtaining the Society's Charter, and he arranged to bequeath the sum of £35,000 for the establishment of four "Linnean Fellowships" of the annual value of £400 each. In 1874, Sir William Macleay bought and fitted out the barque *Chevert* for a scientific expedition to New Guinea; and he was thus enabled to get together a very valuable collection of natural history specimens, which now form an important part of what is known as the Macleay Museum of Natural History, presented by him to the University of Sydney. In addition to his collection, which was estimated at £23,000, he gave to the University £6000 to provide for the salary of a curator.

THE facts relating to the electrical transmission of power from Lauffen, on the Neckar, to Frankfort, a distance of about 110 English miles, have now been made known. They have been established by means of elaborate tests applied by a jury of experts under Prof. Weber, of Zürich. When 113 horse-power was taken from the river, the amount received at Frankfort through the wires was about 81 horse-power, showing an efficiency, in spite of all possible sources of loss, of 72½ per cent. Prof. Silvanus Thompson, who has called attention to these striking facts, points out that it is now only a question of means whether, at the Chicago Exposition, there will be a transmission through wires of 1000 horse-power taken from the Falls of Niagara.

AN Electrical Exhibition was opened at St. Petersburg on January 23 by M. Vishnegradski, Minister of Finance, who was accompanied on the occasion by M. Durnovo, Minister of the Interior, and a number of distinguished persons. The Finance Minister, in addressing those present, traced the progress that had been made in electro-technical knowledge during the last twenty years, and dwelt upon the value of the present Exhibition for students of electricity. The Ministers and the other personages then proceeded to visit the different sections. The Exhibition is said to be of a varied and interesting character, displaying many different kinds of machines at work.

ACCORDING to the American journal *Electricity*, the plans and specifications for the construction of the conduit system and subways in which the electric conductors, at the Chicago Exposition, are to be carried through the grounds to the different buildings have been issued by the construction department of the World's Fair. The specifications call for the completion of the work by April 15, 1892. The total length of the subway is about 4500 feet. The larger portion of the conduit will be 8 feet and 4 inches square, and will be built of the best seasoned pine. The conduit is to have two linings, the outer one consisting of 2 inch tarred plank. Between the linings will be a concrete mixture of cement, plaster, and sand.

THE *Kew Bulletin* for January opens with some most interesting notes, by Mr. J. G. Baker, F.R.S., on Agaves and Arborescent Liliaceæ on the Riviera. Mr. Baker went in November last to the Riviera, chiefly for the purpose of studying these two groups of plants, which grow there in quantities in the open air. The number also contains accounts of the Cape Town Botanic Garden and the Gold Coast Botanical Station.

THE first appendix of the *Kew Bulletin* of the present year consists of a list of such hardy herbaceous annual and perennial plants, as well as of such trees and shrubs as matured seeds under cultivation in the Royal Gardens, Kew, during the year 1891. These seeds are available for exchange with colonial, Indian, and foreign Botanic Gardens, as well as with regular correspondents of Kew. The seeds can be obtained only in moderate quantity, and are not sold to the general public. No application, except from remote colonial possessions, can be received for seeds after the end of March.

THE Woolhope Club has voted £10 towards defraying the expenses connected with the course of Oxford University Extension lectures, now being delivered in Hereford by Mr. C. Carus-Wilson, on the "Outlines of Geology." It is satisfactory to note this instance of a local Club making use of the facilities offered by University Extension for giving to its younger members the opportunity of obtaining systematic training in geological knowledge. The liberal-minded action of the Woolhope Club might well be followed by other Societies throughout the country if they are satisfied of the lecturer's capacity.

WITH reference to Prof. Ray Lankester's communication on "Science in Japan" (p. 256), and especially to his remark that "English, indeed, appears to be the official language of the Imperial University, Tokyo," the following extract may be found interesting. It is taken from the preface to an English translation of a Japanese text-book of elementary geometry, based on that of the Association for the Improvement of Geometrical Teaching, and compiled by Prof. Kikuchi, of the Imperial University, for use in the ordinary normal and middle schools. "In some schools, text-books in English are in use in all the classes, in others only in the higher classes. My object in making this translation is to supply a text-book in English for use in such schools uniform with the Japanese text-book, so that the scholars may pass from one to the other without any trouble."

JUDGING by the contents of a short paper read recently before the Linnean Society, and the discussion which followed, there is an interesting field for scientific investigation amongst the ticks (*Ixodidæ*) which are to be found in some parts of Jamaica and other portions of tropical America. These undesirable Acarina appear to have been introduced to Jamaica with cattle from the mainland. They are most prevalent, therefore,

in districts where cattle-rearing is the principal industry. Their maximum appearance depends very much on the season and other circumstances not yet fully worked out. In tropical countries nearly everywhere there are forms locally called ticks, but evidently allied to the harvest-bugs of Europe. These are called by the French *Rouget*, and in the West Indies *Bête rouge*. They are supposed to be larval forms of Trombidum, and are not ticks in the usual acceptance of the term. One remarkable power possessed by the *Ixodidæ* is that of existing for a great length of time without food. Specimens have been known to live for years accidentally shut up in a small box. Sir Joseph Hooker, in the "Himalayan Journals," recently reprinted, states (p. 196), "that ticks were present everywhere in the hill forests"; and he remarks: "What ticks feed upon in these humid forests is a perfect mystery to me, for from 6000 to 9000 feet they literally swarmed where there was neither path nor animal life." In attacking man and animals ticks insert the proboscis deeply without pain. Buried head and shoulders, and retained by a barbed lancet, they are only to be extracted by force, which is very painful. At present very little is known of the *Ixodidæ* of tropical America. It is possible there may exist numerous species, each with its own special life-history. No one appears as yet to have given undivided attention to the group, and possibly less is known of ticks from a scientific point than any other members of the West Indian fauna. In view of the influence of their occurrence on man and animals this is somewhat anomalous.

M. JEAN DYBOWSKI contributes to the current number of *La Nature* a sketch of a journey he has made from Loango to Brazzaville, and from thence to Bangui. He has collected many objects of scientific interest, including 480 ethnographic specimens, 550 botanic specimens, 280 birds, 100 mammifers, reptiles, fishes, insects, &c.

SEVERAL shocks of earthquake were felt at Rome on the evening of January 22. According to a telegram sent through Reuter's Agency, they caused such a panic in the more crowded quarters that many of the inhabitants fled from their houses, and, notwithstanding the cold weather, spent the night in the streets and public squares. The shocks were felt in the theatres, but the panic there was of short duration. The seismic disturbance had a distinct effect upon the clocks, some stopping at 11.25, and others at 11.27. Several of the lamps in the streets were extinguished. The shocks were noticed by the Pope, who sent to the Vatican Observatory to make inquiries. They were very generally felt throughout the province of Rome. At Genzano a few houses fell in, but no one was injured. At Civita Lavinia an old tower, dating from the Middle Ages, fell and buried two persons, who were, however, promptly extricated. Several houses are in a dangerous state. A severe shock was also experienced at Velletri, but the damage done was insignificant.

ACCORDING to the Paris correspondent of the *Daily News*, two slight earthquake shocks were felt on Sunday, January 24, at Le Mans, the centre of an important agricultural district in the west of France. At Sarce, about 2 a.m., the villagers were awakened by a rather severe shock which caused the school bell to ring. At Château du Loir, a town on the State railway line from Paris to Bordeaux, the first shock lasted three seconds, and awakened everybody. The second took place at half-past three, and was slight.

THE U.S. *Monthly Weather Review* for October 1891, contains a continuation of curves previously published, showing the fluctuations of temperature and pressure at the base and summit of Mount Washington (altitude 6279 feet), and completes them for the months January to March from 1871–86, or for 16 years, with a short discussion by Prof. H. A. Hazen. The base curves show many minor fluctuations of temperature not to be found in the summit curves, most of which are probably due to diurnal range, but as regards the larger fluctuations the most marked characteristic in the temperature curves has been their closeness at base and summit. The earlier change at the summit in both cold and hot waves is remarkable. The fluctuations of pressure are almost identical at the base and summit. Occasionally, the change in temperature at the summit has preceded that in pressure to such an extent as to cause the phases of the latter to lag behind. The curves have been published in the hope that meteorologists will make a special study of them.

IT is a well-known fact that, with the same temperature by the thermometer, one may have, at different times, a very different feeling of heat or cold. This varies with the temperature of the skin, which is chiefly influenced (according to M. Vincent, of Uccle Observatory, Belgium), by four things: air-temperature, air-moisture, solar radiation, and force of wind. M. Vincent recently made a large number of observations of skin-temperature in the ball of the left hand, and constructed a formula by means of which the skin-temperature may be approximately deduced from those four elements. He experimented by keeping three of the four constant, while the fourth was varied, and a relation could thus be determined between the latter and skin-temperature. One fact which soon appeared was, that the relative moisture of the air has but little influence on skin-temperature. It was also found that for every 1° C. of the actinometric difference (excess of black bulb thermometer) the skin-temperature rises about 0°·2; and with small wind-velocities, every metre per second depresses the skin-temperature about 1°·2. In testing his formula, M. Vincent found, with cold or very cold sensation, considerably greater differences between the calculated and observed values than in other cases. This he attributes to the great cooling of the relatively small mass of the hand. Taking the cheek or eyelid, the results were better. He constructs a scale of sensations corresponding to different skin-temperatures as he found them (which scale would, of course, vary somewhat with individuals).

LAST winter, in December and January, M. Chaix made a number of observations of the temperature of the air, the snow, and the ground, at Geneva; of which he has given an account to the Physical Society there. He observed the air at four different heights; granular, pulverulent, and bedded snow, on the surface and at different depths; and the surface of bare ground as well as of ground covered with snow. There was no difference in mean temperature between the air at 1 and at 2 metres; and very little between the former and that on the snow surface. The surface of the ground was 4°·265 C. warmer than the surface of the snow (0·13 m. above), through arrest of radiation. But the bare ground was not cooled so much as the snow surface, and it was only 2°·04 colder than the snow-clad ground. This shows the frigorific influence of snow on climate. Air passing over bare ground would have been 2° warmer than if it passed over the snow. The snow surface was sometimes warmer, sometimes colder, than the air 1 or 2 m. above. In the dry winters of Siberia and Sweden, the snow-surface is generally (according to Woeikof) much colder than the air. M. Chaix explains the variations observed at Geneva by fluctuations in the relative humidity, involving alternate vaporization and condensation at the snow surface. In two-thirds of the cases, indeed, abnormal cooling of the snow corresponded with a low humidity, and heating with a high humidity, and often formation of hoar frost at the surface.

AN interesting paper on Prof. Wiborgh's air-pyrometers was read by Mr. John Crum before the Institution of Engineers and Shipbuilders in Scotland on December 22 last, and is now printed in the Institution's Transactions. Beginning at the beginning, Mr. Crum explained that a pyrometer is an instru-

ment used in the measurement of high temperatures. In constructing his pyrometers, Prof. Wiborgh followed two principles—first, that a certain quantity of air, when heated, is maintained at the same volume, and the increase of pressure gives a measure of the increased temperature; and second, that the air is maintained unaltered in pressure when the temperature is determined by the change of volume. The two forms of his air-pyrometers may be compared to the two forms of the barometer—the mercurial and the aneroid. The air-pyrometer in its aneroid or metallic form is especially adapted to determine the temperature of the hot blast, the gases from all sorts of furnaces, of distillation-products from retorts, &c.; and generally it will fulfil any demands that may fairly be made upon an accurate instrument for ascertaining temperatures for practical purposes, in cases where the temperature to be determined ranges from 0° to 1400° C.

A REMARKABLE illustration of the height of breaking waves is afforded by the following paragraph, which we take from the *San Francisco Chronicle* of January 6:—"*Portland, January* 5. The lighthouse tender *Manzanita* reached Tillamook Rock Sunday for the first time in six weeks, and brought away the keeper, George Hunt, who has been on the rock for four years, and has been transferred to the Cape Mars Light. He says, in the storm of December 7 the waves swept clear over the house, washing away their boats, and tearing loose and carrying away the landing platform and tramway, which were bolted to the rock. On the 29th the waves were still higher, and streams of water poured into the lantern through the ventilators in the balloon top of the dome, 157 feet above the sea-level. The lighthouse was shaken to its foundation by the impact of seas against it, and the water found its way into the house. Men were on duty all night to keep the lamp burning, and but for the wire screen the shutters of the lantern would have been demolished. All hands were alarmed, and old sailors of the crew say they would sooner have taken their chances on board a ship." Prof. Edward S. Holden informs us that it is known to him personally that this lighthouse is sometimes buried in spray and water, and that the glass of its lanterns has been broken by the impact.

THE U.S. National Museum prints a capital paper, by Mr. Frederic A. Lucas, on animals recently extinct or threatened with extermination, as represented in the Museum's collections. In each case the cause of destruction is noted. Mr. Lucas finds that in nearly every instance the cause is "reckless slaughter by man." As an instance of the way in which animals may be destroyed, he refers in the introduction to peccaries. In 1885 these little animals were so abundant in several counties of Texas that their well-worn tails were everywhere to be seen, while their favourite haunts could be readily picked out by the peculiar musky odour characteristic of the creatures. Shortly after that date, hog-skin goods being in favour, a price of fifty cents each was offered for peccary hides, with the result that by 1890 the peccaries were practically exterminated.

THE fresh-water sponges in the collection of the late Mr. Henry Mills were placed some time ago in the hands of Dr. D. S. Kellicott, on the understanding that a representative set of specimens would be selected and prepared for the Buffalo Society of Natural Sciences. Dr. Kellicott has now finished his labours, and submitted the specimens to the Society. He is of opinion that the region about Buffalo Bay and the Niagara River affords almost ideal conditions for the life and growth of fresh-water animals. Its richness, he thinks, is even yet scarcely appreciated. The outlet of the American fresh-water ocean remains at almost a constant level. It is not, like so many lesser American rivers, a mere thread of heated water in summer and a flood of tilt in winter and spring. Neither storm nor season greatly disturbs its clearness or destroys its purity; and owing to its mass its temperature changes slowly and the range is moderate. There are also deep passages, once portions of the river-bed, now almost land-locked, but still sufficiently open to the river to admit fresh water and maintain a constant level. Aquatic life in these channels is remarkably luxuriant for a North American station in latitude 43°. Again, there are small rivers or creeks entering the main river, the estuaries of which are deep, quiet, and supplied from above with swamp and land drainage, whilst their constancy is assured by that of the Niagara. These are teeming with a vast variety of microscopic plants and animals from early summer to December. These conditions are especially favourable to the growth of sponges, and they are found in extraordinary abundance. Whilst the number of species recorded compares favourably with any explored locality in the world, the abundance of representatives is, according to Dr. Kellicott's experience and the testimony of others, quite unsurpassed.

THE working of mercury mines appears to have become an industry of some importance in Russia. According to the *Journal de St. Pétersbourg*, quoted by the *Board of Trade Journal*, there have been found in the district of Bakhmont (province of Catherinoslaw) rich deposits of mercury ore, and the works which have been established there, increasing their operations year by year, have succeeded in producing at the present time 20,000 pouds of mercury annually (poud = 36 pounds avoirdupois). Other deposits of mercury have been discovered in Caucasia, in the province of Daghestan; and the Mining Administration has every reason to believe that private enterprises will be established which will make undertakings of this kind very profitable. Mercury is, as is known, very rare. It is only found in more or less considerable quantity in Spain, Austria, the United States of America, and Italy. The works in the province of Catherinoslaw extract 20,000 pouds of pure mercury from more than 3,500,000 pouds of ore (sulphate of mercury). This quantity is sufficient for Russian consumption, and even allows of an export of 14,000 pouds to other countries.

SOME time ago the Minister of Agriculture in Victoria, acting on the advice of the Board of Viticulture, authorized, among other reserves throughout the colony, the reservation of about 1100 acres in the Dunolly district as a viticultural reserve for experimental purposes. From this area a space of twenty acres has been excised, which has now been cleared and fenced with vermin-proof wire, for the purpose of establishing a perfume farm and experimental plot, for the growing of perfume plants, medicinal drugs, and the production of essential oils. A list of the plants under cultivation at the farm, with a report of the progress up to date, is included in a hand-book issued by the Victorian Royal Commission on Vegetable Products; and it is anticipated that satisfactory results will follow during the present season. The hand-book is published in order that all who may choose to avail themselves of the opportunity may make experiment in their several localities, with the view of comparison with the plot established at Dunolly for educational purposes, and so that an interesting and profitable industry may be established in the colony.

FROM a series of experiments made a short time ago (*Naturw. Rdsch.*), Prof. Wesendonck, of Berlin, inferred that dustless air, in friction with metals, does not generate electricity. But carbonic acid, under like conditions, readily gave a charge, and this was thought to be due to cloud-formation in the gas streaming out of the vessel which had held it in liquid form, the small water-particles charging the metal by friction. Further experiment has seemed to confirm this view. The gas let out from such a vessel, in vertical position, with some freedom, appears cloudy. Gaseous carbonic acid, under 50 or 60 atmospheres,

at ordinary room temperature, must be allowed to issue in a stronger current from the containing bomb to obtain this cloudy look. If a brass spiral is attached to the mouth of the bomb, the cloud-formation is made very difficult; and if the spiral is then strongly cooled, the cloud reappears. With the spiral in boiling water, no cloud is formed, however free the stream of gas. This behaviour quite corresponded to the electrical effects. Even a weak stream of gas from the vessel of liquid carbonic acid gave a well-marked charge; a less effect was had with the bomb of compressed gas; still less when the spiral was added; and least of all with the spiral in hot water, however violent the stream of gas. Prof. Wesendonck concludes that gaseous carbonic acid is not capable of generating electricity by mechanical friction on metal.

HERR ÅNGSTRÖM has been lately engaged in examining with a bolometer the heat radiation of various rarefied gases under the electric discharge. He confined himself to the stronger positive light, using cylindrical glass tubes, with lateral electrodes, and rock salt plates at the ends. An accumulator of 800 Planté elements was the source of electricity. Briefly stated, the results are these:—With a given pressure the radiation is proportional to the intensity of the current. With constant current, the radiation does not vary while the pressure varies between 0·1 and 1·5 mm., but at higher pressures it increases somewhat. With the same gas and pressure, the composition of the radiation is constant, and does not depend on the intensity of the current. With varying density of gas, the ratio of the intensity of radiation of shorter wave-lengths to that of the whole decreases with increase of pressure. (This ratio varied, *e.g.*, from 46 to 15 per cent. in carbonic oxide between the above pressure limits.) Thus this ratio, at low pressures, reaches much higher values than in our ordinary light sources. The intensity of total radiation varies considerably in different gases, and stands in no simple relation to the molecular weight, nor to difference of potential in the gas; nor does it seem to depend on absorption of gas at ordinary pressure and temperature.

DR. SYMES THOMPSON will deliver Gresham Lectures, on the nerves, on February 2, 3, 4, and 5. They will begin each evening at six o'clock, and will be free to the public.

PROF. H. G. SEELEY, F.R.S., will deliver a course of four lectures at Gresham College, in connection with the London Geological Field Class on the four Saturday afternoons in February. The subject will be: "The Physical Geography of the London District in relation to its Geological Structure." Particulars may be had of the Hon. Sec., Mr. J. Herbert Hodd, 78 Queen's Road, Finsbury Park.

THE third series of lectures given by the Sunday Lecture Society begins on Sunday afternoon, January 31, in St. George's Hall, Langham Place, at 4 p.m., when a lecture will be delivered by Mr. Sergius Stepniak. Lectures will subsequently be given by Dr. Andrew Wilson, Mr. George Wotherspoon, Mrs. Proctor (widow of the late Richard A. Proctor), Mr. Frank Kerslake, Miss Amelia B. Edwards, and Dr. E. E. Klein, F.R.S.

THE following gentlemen have arranged to give lectures at the Royal Victoria Hall during February: on the 2nd, Dr. James Edmunds on "An Emigrant in North-West Canada"; 9th, Prof. Oliver on "The Habits of Plants"; 16th, Prof. Carlton Lambert on "Gas, Paraffine, and Electricity" (with experiments); 23rd, Mr. J. W. Gregory on "Waterfalls."

AT the last meeting of the Chemical Society, on January 21, Prof. Smithells gave a preliminary account of some novel experiments on "The Origin of Flame Coloration." At a previous meeting he described a method of widely separating the two cones of combustion which constitute the flame of a bunsen burner (see NATURE, vol. xlv. p. 214). Trying the effect of introducing metallic salts into the two cones separately, he has found that in most cases no marked differences of coloration are produced. But in the case of copper salts the inner cone assumes merely a general yellowish luminosity, whilst the outer cone is brilliantly tinged with the green colour commonly ascribed to the vapour of copper or copper salts. Of the two cones the inner one is by far the hotter. The chief difference between them, apart from this, is that the inner one is surrounded by an atmosphere containing carbon dioxide, carbon monoxide, water, and hydrogen, but no uncombined oxygen, whereas the outer one is bounded by atmospheric air. The only explanation of the phenomenon that has yet offered itself is that the production of the green colour is connected with the act of oxidation. Further support is lent to this view by the fact that if copper oxide dust be introduced into the inner cone, a general luminosity devoid of green is produced, but at the same time the outer cone is coloured green. It would appear as if the copper oxide were reduced to metal in the inner cone, and simply glowed as a solid body, the copper being thereupon re-oxidized in the upper flames in contact with the air. The hypothesis is therefore tentatively put forward that some flame colorations at any rate are due to ether disturbances accompanying the act of chemical combination, and are not to be ascribed to the mere incandescence of single substances. Further experiments made with the apparatus are conformable to this view, but Prof. Smithells has commenced a spectroscopic study of the subject, and has in view the prosecution of independent methods of inquiry. Understanding that the flame-dividing apparatus is likely to come into general use, he has been led to give this preliminary account of the experiments.

A NEW liquid compound of carbon, oxygen, and chlorine was described by M. Troost on behalf of M. Fauconnier at the last meeting of the Académie des Sciences. It may be considered as oxalyl chloride, $\begin{matrix} COCl \\ | \\ COCl \end{matrix}$, the dichlorine derivative of oxalic acid, and has been prepared by M. Fauconnier by the action of phosphorus pentachloride upon ethyl oxalate. Prof. von Richter has previously shown that when these two substances are allowed to react upon each other, a compound of the composition $\begin{matrix} COCl \\ | \\ COOC_2H_5 \end{matrix}$ is formed. This substance, which has been termed chloroxalic ether, is a fuming liquid possessing a pungent odour, and boiling at 131°·5. The new compound is produced by varying the conditions of Prof. Richter's experiment in the following manner. A mixture of phosphorus pentachloride and ethyl oxalate, in the proportion of two molecules of the former to one of the latter, is heated by means of an oil-bath in a flask fitted with a Le Bel-Henninger fractional distillation apparatus and condenser. When the temperature of the bath reaches 125°, a lively reaction commences, accompanied by evolution of ethyl chloride vapour and hydrochloric acid. When the temperature is slowly raised to 150°–155°, a liquid mixture distils over, consisting of oxalyl dichloride, phosphorus oxychloride, and ethyl chloride. When this mixture is subjected to repeated fractional distillation, the oxalyl dichloride is eventually isolated as a mobile, strongly-fuming liquid boiling at 70°. It is endowed with an odour more irritating even than those of the chlorides and oxychloride of phosphorus, and which reminds one somewhat of carbonyl chloride, $COCl_2$. It reacts violently with water, forming oxalic and hydrochloric acids. With anhydrous alcohols it reacts in an extremely energetic manner. Thus with methyl alcohol it forms methyl oxalate, which may be obtained crystallized from the solution, and hydrochloric acid is evolved, great rise of temperature being manifested during the reaction. The formation of this second oxychloride of carbon is of considerable interest, as emphasizing once more the disap-

pearance of the line of demarcation between organic and inorganic compounds; for here we derive what may truly be considered as an inorganic compound from a substance so purely organic as an ethereal salt.

IN our chemical note of last week the experiments of Dr. Merz upon magnesium nitride, Mg_3N_2, were described. It will be remembered that magnesium was shown to combine with nitrogen in a most vigorous manner when heated to redness in a stream of the gas. M. Ouvrard, in the current number of the *Comptes rendus*, shows that lithium too combines energetically with nitrogen. A quantity of this metal was placed in a small boat constructed of iron, the only convenient substance which will withstand the action of fused lithium, and the boat was placed in a combustion tube through which a stream of nitrogen was driven. Upon gradually raising the temperature of the tube and contents, a point was attained, in the neighbourhood of low-redness, when combination suddenly occurred, the metal becoming brilliantly incandescent and increasing rapidly in volume, while the nitrogen in the apparatus was almost entirely absorbed. On continuing the stream of nitrogen until the apparatus became quite cold, the lithium nitride was found in the form of a black spongy mass. Its composition was proved by analysis to be Li_3N, analogous to magnesium nitride, Mg_3N_2, and to ammonia, H_3N. Indeed, it may readily be converted into the latter gas by heating it in a stream of hydrogen. It behaves with water very similarly to magnesium nitride, at once decomposing that liquid with liberation of large quantities of ammonia and formation of a solution of lithia.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus* ♂ ♀) from India, presented respectively by Mr. B. H. Heald and Mrs. E. Day; a Rhesus Monkey (*Macacus rhesus*) from India, presented by Mr. Alfred J. Hayward; two Common Squirrels (*Sciurus vulgaris*), British, presented by Master Fred Corfield; two Ring-necked Parrakeets (*Palæornis torquatus*) from India, presented by Miss Heinekey; six Mantell's Apteryx (*Apteryx mantelli*) from New Zealand, deposited.

OUR ASTRONOMICAL COLUMN.

WOLF'S NUMBERS FOR 1891.—*Comptes rendus* for January 18 contains a communication by M. Rodolf Wolf on the state of solar activity in 1891. The following table shows the results of solar observations made at Zurich Observatory, and magnetic observations made at Milan. The relative numbers (r) have been obtained by the method used in previous years.

1891.	r.	Increments on the relative numbers in 1890.	Variations of magnetic declination.	Increments on the variations of declination in 1890.
January	17·1	11·0	3'·71	0'·69
February	23·0	22·1	4·51	−0·30
March	10·0	4·7	7·85	0·36
April	19·4	17·9	10·58	1·90
May	43·2	38·6	10·70	3·00
June	48·7	47·3	10·36	1·52
July	59·1	47·7	10·98	2·41
August	32·6	24·9	9·96	1·96
September	52·1	35·4	8·55	1·45
October	50·4	39·3	8·49	−0·23
November	41·0	33·8	4·73	1·63
December	30·6	23·4	8·85	0·31
Means	35·6	28·8	7·77	1·22

The relative numbers and the magnetic variations show a decided increase on the values obtained for 1890, and the parallelism between the two series continues as in former years. A discussion of these and previous results indicates that the last minimum of solar activity has the date 1889·6.

A NEW JOURNAL.—The *Sidereal Messenger* has ceased to exist under this title, and has merged into *Astronomy and Astro-Physics*. The first copy of this new journal has recently been published in America. Its production is the natural result of the development of astronomical physics. One portion of the journal is to be devoted to general astronomy, whilst the other deals with astro-physics. The editor of the former is Mr. W. W. Payne, who so ably conducted the *Sidereal Messenger*, and the latter section is under the charge of Prof. G. E. Hale, whose excellent works on solar prominence photography are known to all spectroscopists. If the editors can fill future numbers of the journal with so many interesting and important articles and notes as make up the first number, they will attain a well-deserved success. Many of the articles have been published in other journals, but this, of course, does not in the least detract from the value of the new journal. The literature of spectroscopy is truly said to be widely scattered, and Prof. Hale is doing a meritorious work in bringing it all together.

KOREA.

AT the meeting of the Royal Geographical Society on Monday night, the paper read was on a journey through North Korea to the Ch'ang-pai Shan, by Mr. Charles W. Campbell. Ch'ang-pai Shan, or "Ever White Mountain," is the same as Peik-tu San, or White Head Mountain, and "The Long White Mountain," so graphically described by Mr. James in his book of that title. It lies in Manchuria, just beyond the Korean boundary, and is remarkable for the deep-blue lake which lies in a deep hollow on the ridge joining two of its peaks. It was not till August 1889 that Mr. Campbell succeeded in leaving Seoul, the capital of Korea. He journeyed east and north along the coast. The country traversed is typical of the centre and north of the country.

"Korea," Mr. Campbell said, "is a land of mountains. Go where you will, a stretch of level road is rare, and a stretch of level plain rarer still. The view from any prominent height is always the same; the eye ranges over an expanse of hill-tops, now running in a succession of long billowy lines, now broken up like the wavelets in a choppy sea, often green with forest, but just as often bare, brown, and forbidding. Clear mountain brooks or shallow streams rushing over beds of gravel are never wanting in the valleys below, where a rude log bridge, or curling smoke, or the presence of cultivation, leads you to observe the brown thatch of some huts clustered under the lea of a hill. These hamlets are of two distinct kinds—the purely agricultural, and those which depend as much on the entertainment of travellers as on farming. The site of the agricultural village is a hill-slope facing the south. Over this, low, mud-walled, straw-thatched hovels, each standing in its own patch of garden, which is protected by a neat fence of interlaced stems, are scattered at random, and there is not much attempt at a street anywhere. Every house has its threshing-floor of beaten clay, the workshop of the family. The stream which runs past the foot of the hill, or courses down a gully in its side, is lined with women and girls washing clothes with sticks instead of soap, preparing cabbages for pickle, or steeping hemp. Seen from a distance, these places are quite picturesque. The uneven terraces of thatch are brightened by the foliage and flowers of gourds and melons which climb all over the huts. In the gardens surrounding each house are plots of red chilli, rows of castor-oil plants, and fruit trees, such as peach, apricot, pear, and persimmon.

"The roadside village, on the other hand, is generally a most unlovely spot. The only street is the main highway, which is lined on both sides by a straggling collection of the huts I have mentioned. Heaps of refuse, open drains, malodorous pools, stacks of brushwood for fuel, nude sun-tanned children disporting themselves, men and women threshing grain, and occasionally a crowd of disputants, all combine to make it a very indifferent thoroughfare. Most of the houses are inns or eating-shops. The main gate of the inn leads directly from the street into a quadrangle bounded on two sides by open sheds, which are provided with troughs for the feeding of pack animals, and on the other two sides by the guest rooms and kitchen. The courtyard is untidy, often dominated by a powerful pig-stye, and littered with fodder or earthenware pitchers and vats, whose contents are usually the strong-smelling pickled cabbages and turnips so dear to Korean stomachs.

"The main industry, of course, is agriculture, carried on under

disadvantages inseparable from the mountainous character of the country. In Japan and China we know that persevering care and energy have overcome similar disadvantages, but it is not so in Korea. The terrace cultivation, the irrigation works, and above all the patient, almost fastidious labour, which make the hills of Japan and South China yield their share of the earth's good fruits, are practically unknown. Where water is abundant and easily manageable, the lower reaches of the valleys are taken up with rice, the higher portions with millet, beans, buckwheat, &c. A particularly favourable slope, all the better if it faces the south, is usually as much as the sides of the valley are called upon to contribute to cultivation. There is considerable waste about the paths and paddy-dykes, weeds are rank and numerous, and the prim neatness so conspicuous in Japanese farming is entirely wanting. Much of the newly broken ground is naturally stony, and little effort is exercised to make it less so. However, considering the small amount of labour expended on agricultural operations, the crops are good, and speak eloquently for the fertility of the soil."

Mr. Campbell reached the River Yalu in October, and although he made every endeavour to reach his goal, the snow was so deep, the passes so overhung with accumulations of snow, and his guides so terrified, that he was compelled to turn back when within a mile or two of the summit. Nevertheless, he succeeded in making observations of considerable interest.

"Peik-tu San, or Lao-pai Shan (Old White Mountain) as it is at present called by the Chinese of Manchuria, is the most remarkable mountain, naturally and historically, in this part of Asia. The perennial whiteness of its crest, now known to be caused by pumice when not by snow, made the peoples that beheld it from the plains of Manchuria give it names whose meanings have survived in the Chinese *Ch'ang-pai Shan*, or Ever White Mountain. This designation, obviously assigned to the White Mountain alone, has been extended to the whole range without apparent reason, for no other peak of it, so far as is known, can pretend to perpetual whiteness, whether of pumice or snow. Some 100 miles south-east of Peik-tu San there is a Ch'ang-peik San (Ever White Mountain) which must approach, if it does not exceed, the White Mountain in height, but the Koreans do not credit it with a snowy covering for more than nine months of the year, and a European traveller who has seen it informs me that it is wooded to the summit, quite unlike Peik-tu San, which is bare of forest for the last 1000 feet of its height. The great point of interest in the mountain, apart from its whiteness, is the lake—12 miles in circuit according to Mr. James and his party, the only Europeans who have seen it—which lies in the broad top of the mountain at a height of 7500 feet above sea-level, and is supposed to be the source of the three rivers, Yalu, Tumen, and Sungari. The *Tei Tei-ki*, Great Lake, as the Koreans call it, is the nucleus of a mass of legend and fable. It is a sacred spot, the abode of beings supernatural, and not to be profaned by mortal eye with impunity. Curiously enough, neither Chinese nor Koreans have the faintest notion of the real character of Peik-tu San. The Chinese say that the lake is an 'eye of the sea,' and the Koreans tell you that the rock of which the mountain is composed floats in water, for lumps of pumice were common on the Yalu at Hyei-san." Mr. Campbell's crude geological explanations, that this *cho-san* (ancestral mountain) of Korea was a burnt-out volcano, whose crater had been filled with water by springs, were listened to with polite wonder, and treated with much less credulity than they deserved. He pointed to the black dust, to the clinkers, and to the rocks lining the banks of the Yalu for miles, many of which looked as if they had been freshly ejected from some subterranean furnace, bnt to no purpose. If the occurrences he spoke of had taken place, they must have been handed down by tradition; and it was useless to cite lapse of time—Koreans are ignorant of geological periods—to people whose history extends as far back as 4000 years ago. According to Mr. Campbell's observation, most of the forest between Po-ch'ön and Peik-tu San grows on volcanic matter, which was without doubt ejected from Peik-tu San during successive eruptions. The general inferiority of the timber hereabouts to that which he saw elsewhere in Korea led him to examine the soil wherever an uprooted tree or a freshly-dug deer-pit furnished the opportunity. "Beyond a thin coating of leaf-mould on the surface, there was seldom anything else but pumice, broken to the size of a very coarse sand. According to the hunters, this was the subsoil everywhere in the forest, and to my knowledge it extends for forty miles at least to the south from Peik-tu San. Nearing the mountain we get the clearest evidence of the character and recency, geologically speaking, of the eruptions which spread this vast quantity of volcanic material over such a wide area. Ten miles due south of the White Mountain, the Yalu, now 8 or 10 yards broad and very shallow, flows between banks like a railway-cutting, sheer, clean, and absolutely devoid of vegetation, for denudation was too rapid to permit the slightest growth." The sections thus exposed were often over 100 feet in depth, and at one of the deepest portions Mr. Campbell counted thirteen layers of black volcanic dust, all varying in thickness, and each separated from the layer above by a thin layer of light-coloured mould. So fine was this dust that the least breath of wind caught it and scattered it freely over the adjoining snow, to which it gave a grimy, sooty appearance.

"The forests of South Manchuria, though uninhabited now, were, we learn from Chinese records, the home of many races in ages past. The comparatively recent kingdom of Ko-ku-rye, which arose in the first century B.C., is said to have occupied the Ch'ang-pai Shan and the head-waters of the Yalu river. Anyone who has travelled through the forests might be inclined to doubt such records, for, excepting hunters' lodges, one never notices a vestige of human occupation. But it must be remembered, on the other hand, that the word *kuk* (Chinese *kuo*), country or kingdom, was applied in the early history of Korea and Manchuria to very limited communities, often to mere villages. The word "tribe" better expresses what the so-called kingdoms actually were; and when we bear in mind their low civilization and the impermanent character of their dwellings, it is not surprising that my hasty journey failed to throw any light on the ancient inhabitants of these forests." Since his return, however, Mr. Campbell was informed by Mr. Fulford that Chinese hunters told him of the discovery by them of human implements—of what kind Mr. Campbell cannot say—when digging deer-pits near the White Mountain.

Mr. James, in a paper read before the Royal Geographical Society in June 1887, described very fully the guild of hunters which practically owns and rules the forests to the north and west of Peik-tu San. The Koreans have no such guild, probably because they have not so much to fear from bandits, but each hunter has a recognized right of ownership over a rudely defined district in the neighbourhood of his hut. Over this he hunts and traps deer in summer, and sable at the beginning of winter, altogether spending about five months of the year in the forest; the remaining seven are passed at his home on or near the Yalu, either tilling his ground or living in idleness on the proceeds of hunting seasons. Besides sable and deer, tiger leopard, bear, pig, and ermine are found here; bear, probably the common brown species (*Ursus arctos*), are said by the hunters to be very numerous in summer. In mid-Korea Mr. Campbell has seen a small black bear with a white patch on his chest (*Ursus tibetanus*), but the Yalu trappers did not seem to know it. Hazel-grouse were the only game-birds he noticed. Throughout the forests insect pests abound in the summer months. Mosquitoes, gnats, and gad-flies make the lives of the settlers perfectly burdensome for two or three months of the year, and ponies and bulls quickly succumb to their attacks. The houses are kept constantly filled with birch-smoke to drive them off; cattle are protected by fires of greenwood in the open; and men working the clearings carry coils of rope made from dried *Artemisia*, which burns slowly and emits a pungent odour, for the same purpose.

THE GEOLOGY OF THE HIMÁLAYAS.

THE twenty-third volume of the Memoirs of the Geological Survey of India, consisting of some 250 pages, is wholly taken up by an account of the geology of the Central Himalayas, by the Superintendent of the Survey, Mr. C. L. Griesbach, C.I.E. The carefully written text is illustrated by some of the most exquisite and instructive photographs of synclinals, folded beds, faults, glaciers, &c., which have ever been produced, to say nothing of the numerous maps and sections.

We have thought it best to give Mr. Griesbach's conclusions on the important subject with which he deals in his own words:—

The Himálayan region forms part of the vast structure of the Central Asian elevation; it is so closely connected with the latter, both structurally and geographically, that it is very

difficult to decide its exact limits. Native geographers and the Puranic scriptures define the Himálayas as comprising only the chain of snowy peaks at the head of the Ganges drainage. Modern views generally limit the Himálayas to the system of mountain ranges which extend between the Brahmaputra and Indus rivers. Of course, structurally, these ranges continue beyond these boundaries, but there are distinct changes in the features of the ranges which make these limits advisable. As regards the lateral extension of the region, several views have been formed; but I consider it most convenient, and at the same time more in accordance with the original significance of the term, to call Himálayas only the system of ranges which fringe the Tibetan highlands along its southern margin, a view which is now most generally held. That part of the system in which rise the headwaters of the Ganges drainage, and extending north-westwards as far as the Sutlej gorge, I call here the Central Himálayas, and within this area I divide the Central ranges into (1) Northern range (watershed), and (2) Southern range (line of highest peaks).

Whilst the Southern range of the Central Himálayas is formed chiefly of crystalline rocks, mostly gneissic with metamorphic schists, it is shown that the Northern range is almost entirely composed of a vast sequence of sedimentary strata, ranging from the lowest palæozoic to tertiary and recent age. The detailed description of these various formations I have given in the preceding pages, and I will here only recapitulate the following points.

Immediately on the crystalline schists reposes an enormous thickness of beds of varying lithological character, named haimantas by me, which are sharply defined near its upper limit by most characteristic red quartz shales, which form the base of the richly fossiliferous lower silurians. Structurally, this system is very much more fully developed than the succeeding silurians, being in most sections more than double the thickness of the latter. But the lower limit of the haimantas is obscure; an almost perfect lithological passage may be traced from the crystallines (vaikritas) into this system, both in the western and easternmost sections described.

One of the most characteristic amongst the various horizons in this system is a great thickness of a coarse conglomerate or boulder-bed, which in some sections alternates with slaty beds, but is never entirely absent. This, in conjunction with the ripple-marking which may be seen on nearly all the slaty beds of the haimantas, indicates clearly that we must suppose the ancient coast-limits of haimanta age to have been in close proximity. The apparent overlap of haimantas on gneiss (Niti area) is easily explained, if we suppose this system to have been developed in this region as a littoral formation. It is extremely probable that one of the earliest Himálayan disturbances occurred immediately before haimanta times.

Lithological resemblance, not less than structural features, point to the probability that a part at least of the slate series of the Lower Himálayas are equivalents of the haimanta system of the Central Himálayas. I believe even that some of the older rocks, which immediately underlie the Vindhian group, may yet be found to belong to the same age. It would thus follow that the haimanta seas had extended not only over the greater part of the present Himálayan area, but perhaps also as far south as Central India. If so, the line of the Central Himálayas was probably marked out as a chain of elevations, from the waste of which the boulders and pebbles of the haimanta conglomerate and of the Simla rocks were derived. The latter supposition is also advanced by the authors of the "Manual."[1]

The palæozoic group forms an uninterrupted sequence from the lowest haimantas to the upper carboniferous; and this sequence is the same, or nearly so, in all the sections of the Central Himálayas. The first indications of a disturbance are noticeable in the upper carboniferous. Certain beds of the latter are wanting in some sections, and I found the next following system overlapping what I must look upon as an eroded surface of upper carboniferous.

Nearly everywhere I found the latter overlaid by a great sequence of beds, which represent permian, trias, rhætic, and lias. This group of systems forms an uninterrupted sequence, with conformable bedding throughout. The base of the sequence is everywhere seen to be dark crumbling shales, which contain a palæozoic fauna, probably permian in character, which gradually passes into lowest trias beds through dark limestones and shales which have yielded a curious fauna, some of the species of which have strong affinities with permian forms. On it rest lower trias beds, followed by a continuous succession of strata, which reach up into the lower lias.

The same condition prevails in Spiti, where the lower lias is also well represented.

The lias limestones and shales are overlaid by jurassic (Spiti) beds, which have yielded a large number of fossils, but which have not yet been entirely examined. Most of them appear to belong to the upper jurassics rather than middle or lower. Whether the latter is represented or not, is not quite clear, but the bedding of the Spiti shales is isoclinal with the lower lias, and if there is an unconformity between these systems, it may only be conjectured from the sudden and entire change in lithological character of the two formations, coupled with the absence of lower jurassic forms amongst the species found in the Spiti shales.

From this formation there is a gradual passage into the greenish shales and sandstones of the cretaceous (with perhaps upper jurassic), the Gieumal sandstone of Stoliczka. Again a sudden change in lithological character from these sandstones into the white limestone of the upper cretaceous seems to point to the probability of there having occurred physical changes on a large scale after the deposition of the lower cretaceous. In the Central Asian area, and also in the Perso-Áfghán region, a strongly marked overlap of the upper cretaceous over the neocomian limestones may be observed.

Probably similar features will be found to exist in the Himálayan area, the cretaceous rocks of which have not been closely studied.

The tertiary system is fully developed, though few fossils were found in it. A great unconformity occurs between certain sandstones which cannot be older than upper eocene (overlying *nummulitics*), and are probably of miocene age, and horizontal beds of clay, sand, gravels, and sandstone, which form the high table-land of Húndés, which, having yielded mammalian bone remains, are commonly known as the ossiferous beds of Húndés.

From the foregoing it will be seen that special disturbances must have occurred in early geological times, and have been repeated periodically.

It is very certain that near the beginning of the haimanta era sufficient physical changes have occurred not only to completely alter the lithological character of the deposits in course of formation, but also the area in which the latter were laid down. The great thicknesses of coarse conglomerates, which are of widespread extent in the lower haimantas, indicate the nearness of land at the time, or, as I may term it, the existence of an early region of elevation in place of the present area of the Central Himálayas. At the same time lithological, not less than structural, conditions point to the probability of true haimanta deposits having been laid down also on the south slope of what is now the Central Himálayan region.

The compression of the Himálayan, and indeed entire Central Asian area, and consequent folding, and thus elevating of it, most probably went on uninterruptedly and continuously from the earliest epochs to the present; indeed, the natural forces exerted on the surface of our globe condition this. But in addition to this, periodical greater changes have taken place, and are proved by the sections of the Central Himálayas.

After the lower haimanta recession of deposits from the entire Himálayan area into well-defined northern and southern regions of formations, we find an undisturbed sequence of beds till the upper carboniferous, when clear evidences of a great overlap may be observed. This is well marked in the Central Himalayas, and is clearly proved in the Perso-Áfghán area, where carboniferous marine limestones are followed by littoral deposits, the upper beds of which contain a triassic fauna. Here we have therefore a period of sub-aërial and marine erosion of the carboniferous, followed by an overlap of probably a permian and triassic sequence of deposits.

The third period of disturbance seems to belong to the lower jurassic age, where a gap (partial or otherwise) between lower lias and middle or upper jurassics is probable.

I may mention that this gap is not observable in the Perso-Áfghán region, where the passage from the trias into jurassics and neocomian is gradual.

On the other hand, a decided overlap on an immense scale has occurred in later cretaceous times in Central Asia, and we find that hippuritic limestone covers both jurassics and neocomian unconformably. Such is less apparent in the Central Himálayas, though probable enough when considering the sudden change from the sandstone and shales of the lower cretaceous to the hard white and grey limestone of the upper cretaceous.

The fifth period of disturbance, which is clearly shown in the

[1] Page 679.

Central Himálayas, occurred after the deposition of the sandstones which overlie the *nummulitics* of Húndés, and which are probably of miocene age. A considerable gap seems to exist between the latter and the ossiferous younger tertiaries which fill the Húndés basin.

There is clear evidence, therefore, of very early disturbances having taken place in the Himálayan area. There are abundant proofs that minor changes in the distribution of land and water have occurred not only frequently, but we can scarcely believe otherwise than that the forces which have resulted in the intricate folding and crumpling of the great sequence of sedimentary and crystalline strata must have been of very long duration, and were probably existent from the very earliest date when the first grain of sediment was deposited in the Himálayan seas. We can go further. Whatever other—and as yet only dimly understood—forces were at work to produce this contraction and folding of the earth's crust, we know of two forces about which there can scarcely be the slightest doubt. The first is the gradual cooling of our earth, and consequent lessening and shrinking of the surface of it. Secondly—and this is a force which may be mathematically expressed—we know that the centrifugal force endeavours to move every point on the surface of the earth in a direction opposite to that in which gravitation attracts it.

The actual force exerted is the resultant between the centrifugal and tangential forces, and it has the tendency, if I may so express it, of gradually moving each point on the surface of the earth towards the equator. It may be supposed that an enormous sequence, of to a certain extent pliable deposits, trying to move bodily, as it were, towards the equator, but *en route* arrested and banked up against a rigid mass of which the peninsula of India is a small remnant only, must necessarily have suffered wrinkling, and lateral crushing.

These forces operated since the earth existed, and must be active now. But throughout the great sequence of the palæozoic, mesozoic, and kainozoic deposits, we search in vain for an internal explanation of the great unconformities and disturbances of coast-line which have taken place at certain intervals, such as I have sketched out above. That these changes were not local overlaps only is apparent when we compare the Central Himálayan area with the Perso-Afghán region. In the latter the physical changes are far more clearly marked. At the close of the carboniferous epoch, which was one of pelagic conditions in the Hindu Kúsh area, Khorassán and Persia, the distribution of land and water must have considerably changed, as we find immediately above the carboniferous limestone, shaly beds with coal-seams, and conglomerates and partly littoral, partly freshwater conditions prevailed in that area till late into jurassic times. These disturbances, which are slightly indicated in the Himálayas, are clearly shown and occur on a larger scale in the West Central Asian area.

The next great change in the Perso-Afghán area is the great overlap of the upper cretaceous (hippuritic) limestone over the neocomian, already alluded to. It has resulted in a great and often strongly expressed unconformity. Again, another and strongly marked change occurs in the middle tertiaries of the Perso-Afghán area. The purely marine miocene beds are overlaid, often with isoclinal bedding, at other localities distinctly unconformably, by upper tertiary freshwater deposits. If the folding and crushing process were alone the cause of these—shall I call them cycles of disturbances—then at least some evidence of it should be observable within the sequences of rocks as we see them.

On the other hand, there is no direct evidence to show that the raising of the Himálayas as a mountain system was in any way due to these periodical fluctuations of sea-level, or, as Suess terms it, the "positive" and "negative" movements of the liquid covering of the earth. The evidence of the transverse valleys in the Himálayas points even to the probability that the raising up of the chains of hills forming them, *i.e.* the folding and crumpling of its rock strata, must have kept pace, step by step, with the erosion by rivers which we now find traversing the whole width of this mountain system.

Such transverse valleys, however, can only date since the last of the periodical changes spoken of, *i.e.* since the middle tertiary epoch. Before that time, up to the point when the last marine tertiary deposits were laid down along the margin of the Himálayas, the relative position of Peninsular India and Central Asia must have been the reverse of what we know them to be now; that is to say, the surface of the Central Asian elevated *massif* must have been nearer the centre of our earth than the surface of the continent, of which the Peninsula of India forms only a portion of the remains.

It is improbable that the folding action alone has been the cause of the present structure and orographical features of Central Asia and the areas south of it: for the final great changes which have resulted in the draining of Central Asia of the tertiary seas, of which nothing now remains but isolated salt-water lake-basins, such as the Aral and the Caspian are, we must look for other causes.

Possibly such may be found in the sinking in of large portions of the southern hemisphere which caused the submergence of the Indo-African area below what is now the Indian Ocean. With it the part now known to us as the Peninsula of India may have partially broken down, though of that we have no direct evidence, unless the improbability that the Central Asian area could have been pushed up to its present elevation above the Peninsula entirely through being folded might be adduced as proof. Large tracts of Central Asia we know could never have suffered folding to any but very slight extent, as, for instance, the greater part of the tertiaries of the Turkistán region which are often in undisturbed horizontal position. On the other hand, these latter are but little raised above—some are even depressed below—the level of India.

In all these considerations and speculations two points seem probable almost beyond doubt, namely: First, that the last and main disturbance of physical conditions of the Central Asian area has taken place in post eocene, perhaps in middle tertiary times, and is most likely still continued to the present day.[1] Secondly, that this period of disturbance coincides with the sinking in of the Indo-African continent, which "breaking down" caused the final draining of the tertiary seas from the Central Asian area.

Not so certain is whether the raising *en bloc* of the Central Asian mass above the level of the Indian Peninsula is due only to the folding process, or whether some movement downwards of the Peninsula, in connection with the sinking in of the Indo-African region, may not have had a share in producing the present configuration of the Húndés plateau. Some such movement may be conjectured. Certain supposed elevations of the Peninsula may possibly be owing to "negative" movements of the area of the Indian Ocean—in other words, to the sinking in of the ocean bed.[2]

SCIENTIFIC SERIALS.

American Journal of Science, January.—Theory of an interglacial submergence in England, by G. Frederick Wright. The theory of deep interglacial submergence which has been propounded to account for the shell-beds at Moel Tryfaen, near Snowdon, and at Macclesfield, is opposed by several formidable objections, viz. (1) the subsidence must have been one which affected North Wales and central England without affecting the region south of the the Thames and Bristol Channel; (2) there is in other places a considerable absence of marks of subsidence over the northern part of the centre of England, where it is supposed to have been the greatest; (3) the Pennine Chain is not more than 25 or 30 miles wide from east to west, yet east of Macclesfield there is an entire absence upon its flanks both of glacial deposits and of beach lines; (4) the shell beds are strictly confined not only to the area which was demonstrably covered by glacial ice, but to those more limited areas which were reached by ice that is known to have moved in its way over shallow sea-bottoms; (5) the assemblage of shells is not such as could have occurred in one place in the ordinary course of nature. The author develops a system of glaciers which will explain the facts at present known, upon the supposition of a single glacial epoch.—The Permian of Texas, by Ralph S. Tarr. It is shown that the Permian of Texas is, like other areas of Permian, a deposit in large measure made in an inland sea.—The chemical composition of iolite, by O. C. Farrington. The formula obtained from two analyses of exceptionally pure specimens of the mineral is H_2O $4(MgFe)O$ $4Al_2O_3 10SiO_2$, the ratio of MgO to FeO in the two cases being as 7 : 2.—On a series of cæsium trihalides, by H. L. Wells; including their crystallography, by S. L. Penfield. Upon adding bromine to a concentrated solution of cæsium chloride, a bright yellow precipitate was obtained, from which crystals were formed having the composition $Cs.Cl.Br_2$. An attempt has been made to

[1] "Manual," pp. lvi., 680, &c.
[2] *See* "Manual," p. 681.

prepare all the members in the following series, and, with the exception of Nos. 4 and 10, all of them have been isolated (1) CsI_3, (2) $CsBrI_2$, (3) $CsBr_2I$, (4) $CsClI_2$, (5) CsClBrI, (6) $CsCl_2I$, (7) $CsBr_3$, (8) $CsClBr_2$, (9) $CsCl_2Br$, (10) $CsCl_3$. The characteristics of these compounds have been fully studied.—The law of elastic lengthening, by J. O. Thompson. The author has made an extended and thorough investigation on Hooke's law. The experiments were carried out at the Physical Institute of the University of Strassburg, with the advice and help of Prof. Kohlrausch. They lead to the following conclusions :—(1) The generally accepted law of elastic lengthening, $x = \alpha P$, according to which the lengthening x is proportional to the stretching weight P is only an approximation. (2) The relation between elastic extension and stretching weight can be expressed by an equation of the following form :—

$$\chi = \alpha P + \beta P^2 + \gamma P^3.$$

(3) The modulus of elasticity of the undeformed body can be calculated with the help of the equation

$$\left(\frac{d\chi}{dP}\right)_{P=0} = d.$$

(4) The true moduli of elasticity, calculated in this way, may be as much as 16 per cent. larger than those determined in the ordinary way. Consequently it will be necessary to recalculate physical constants which depend on the modulus of elasticity.—A method for the quantitative separation of strontium from calcium by the action of amyl alcohol on the nitrates, by P. E. Browning.—The relation of melting-point to pressure in case of igneous rock fusion, by C. Barus. From the experiments on diabase the relation of melting-point to pressure at 1200° is $dT/dp = \cdot 021$; at 1100°, $dT/dp = \cdot 029$. And since the probable *silicate* value of $dT/dp = \cdot 25$ at 1170°, and as this falls within the margin (·020 to·030) of corresponding data for organic substances such as spermaceti, paraffin, &c., it is inferred that the relation of melting-point to pressure, in case of the normal type of fusion, is nearly constant, irrespective of the substance operated upon.—The discovery of Clymenia in the fauna of the Intumescens zone (Naples beds) of Western New York, and its geological significance, by John M. Clarke.—A new meteoric iron from Garrett Co., Maryland, by A. E. Foote. A plate accompanies this paper.—Farrington, Washington Co., Kansas, aërolite, by G. F. Kunz and E. Weinschenk.—The skull of Torosaurus, by O. C. Marsh.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 21.—"Additional Observations on the Development of *Apteryx*." By T. Jeffery Parker, B.Sc., F.R.S.

The paper is founded upon the study of three embryos of *Apteryx australis* obtained since the author's former communication on this subject was written.

The youngest (stage E′) is intermediate between E and F of the former paper, the next (F′) between F and G, the most advanced (G′) between G and H.

In E′ the characteristic form of the beak has already appeared.

In F′ the pollex is unusually large, giving the fore-limb the normal characteristics of an embryo wing.

Several important additions and corrections are made to the former account of the skull, especially with regard to the presphenoid region, the basi-cranial fontanelles, and the relations between the trabecular and para-chordal regions.

The account of the shoulder-girdle is amended. In *Apteryx oweni* the coracoid region is solid, and no pro-coracoid appears ever to be formed : in *A. australis* a ligamentous pro-coracoid is present at a comparatively early period (stage F′, and perhaps E′).

An intermedium is present in the carpus in all three specimens, in addition to the elements previously described.

The brain in stage G′ is interesting, as being at what may be called the critical stage ; the cerebellum is fully developed, and the optic lobes have attained the maximum proportional size and are lateral in position. In all essential respects the brain of this embryo is typically avian.

Royal Microscopical Society, January 20.—Dr. R. Braithwaite, President, in the chair.—The Society adjourned after passing a vote of sympathy and condolence to His Royal Highness the Prince of Wales (Patron of the Society) on the sad loss he had sustained.—This being the annual meeting, the President's address, which was to have been read, was therefore postponed till the next meeting, February 17.

EDINBURGH.

Royal Society, January 4.—Prof. Sir W. Turner, Vice-President, in the chair.—Dr. Noel Paton read a paper on the action of the auriculo-ventricular valves. It has hitherto been supposed that, when these valves close, the two flaps are floated up by the fluid, and, partially overlapping, prevent the passage of the fluid by being pressed against each other. Thus it has been supposed that, when closed, the upper surface of one flap presses against the under surface of the other. Dr. Paton has found, by direct experiment, that the flaps remain, on the whole, in a pendant position, the upper surfaces of the two being pressed together.—Mr. John Aitken read the second part of a paper on the number of dust particles in the atmosphere of certain places in Great Britain and on the Continent, with remarks on the relation between the amount of dust and meteorological phenomena.—Dr. Thomas Muir read a paper on a theorem regarding a series of convergents to the roots of a number. The investigation was suggested by some work of the late Dr. Sang. The series does not converge rapidly, and so cannot be of great practical use.—Mr. Malcolm Laurie read a paper on the development of the lung-books of *Scorpio*, and the relation of the lung-books to the gills of aquatic forms. He was led to investigate this subject by observations made on the allied fossil forms described in his paper read at the previous meeting of the Society. He concludes that the lung-books are not formed by a process of invagination, as is usually supposed to be the case. He considers that the cavities are formed by the growth of a protecting plate which finally adheres to the body.

SYDNEY.

Royal Society of New South Wales, November 4, 1891.—H. C. Russell, F.R.S., President, in the chair.—The following papers were read :—Notes on Artesian water in New South Wales, by Prof. David.—On the constitution of the sugar series, by W. M. Hamlet.

December 2.—H. C. Russell, F.R.S., President, in the chair.—The following papers were read :—On kaolinite from the Hawkesbury sandstone, by H. G. Smith.—Notes on some New South Wales minerals (Note No. 6), by Prof. Liversidge, F.R.S.—Notes on the rate of growth of some Australian trees, by H. C. Russell, F.R.S.—Some folk-songs and myths from Samoa, translated by the Rev. G. Pratt, with introductions and notes, by Dr. John Fraser.

PARIS.

Academy of Sciences, January 18.—M. Duchartre in the chair.—Obituary notice on the late Sir George Biddell Airy, by M. Faye.—On the mass of the atmosphere, by M. Mascart. It is shown that the determination of the mass of the atmosphere by observations of the pressures at the surface is open to serious objections, and involves a notable error. The mass, calculated by means of the formulæ developed by M. Mascart, is one-sixth greater than that usually obtained. The quantity of air situated at a height of 64 kilometres is 1/700 of the total mass. Particles of ice and water are suspended at this height, although the air is so rarefied. It is therefore presumed that the density does not diminish uniformly with increase of height above sea-level, but decreases more slowly in high than in low strata. [On this point see a note in NATURE, p. 259.]—New note on the resistance and small deformations of helical springs, by M. H. Resal.—On solar statistics for 1891, by M. Rodolf Wolf. (See Our Astronomical Column.)—Observations of Wolf's periodic comet, made in 1891 with the great equatorial of Bordeaux Observatory, by MM. G. Rayet, L. Picart, and Courty. Observations of position are given, extending from June 27 to December 27.—On integrals of differential equations of the first order, possessing a limited number of values, by M. P. Painlevé.—On an arithmetical theorem of M. Poincaré's, by M. Victor Stanievitch.—On organic compounds as solvents for salts, by M. A. Etard.—Action of carbon monoxide on iron and manganese, by M.

Guntz. Pure finely divided manganese, obtained by heating an amalgam formed electrolytically, at 400° completely absorbs pure carbon monoxide as follows :—$Mn + CO = MnO + C$. The reaction is probably the same in the case of iron. This explains the facility with which C is taken up by iron in the blast furnace. The spongy iron reduces CO, and finely divided C is deposited in contact with the FeO formed ; at a higher temperature the FeO is reduced by CO, when the metallic Fe readily takes up the finely divided C intimately mixed with it.—Action of carbon on sodium sulphate, in presence of silica, by M. Scheurer-Kestner.—Lithium nitride, by M. L. Ouvrard (See Notes.)—Action of phosphorus pentachloride on ethyl oxalate, by M. Ad. Fauconnier (See Notes).—On the thermal value of the substitution by sodium in the two alcoholic hydroxyl groups of glycol, by M. de Forcrand.—An isomeride of camphor, by M. Ph. Barbier.—The fixation of iodine by starch, by M. E. Rouvier.—The rotatory power of silks of different origin, by M. Léo Vignon.—Action of boric acid on germination, by M. J. Morel.—Contribution to the embryogeny of *Smicra clasipes*, by M. L. F. Henneguy.—On some new Coccidia, parasites of fishes, by M. P. Thélohan.—On the prevention of hiccough by pressure on the phrenic nerve, by M. Leloir. Five years ago the author was consulted by a girl twelve years of age who hiccoughed every half-minute. She was thus prevented from sleeping, or masticating her food, and her life was despaired of. Anti-spasmodic prescriptions were tried in vain. After pressing the left phrenic nerve, however, for about three minutes, the hiccoughing disappeared. The method has since been successful in many other cases.—On the muciferous apparatus of Laminaria, by M. Léon Guignard.—On the dorsal insertion of the ovules of Angiosperms, by M. Gustave Chauveaud.—On chloride of sodium in plants, by M. Pierre Lesage. It appears that when *Lepidium sativum* and *Raphanus sativus* are watered with a solution of sodium chloride the elements of this salt are found in these plants, consequently a certain proportion of each is absorbed by the plants.—Observation of a lunar corona on January 14, 1892, by M. Chapel.

BERLIN.

Physical Society, January 8.—Prof. Kundt, President, in the chair.—Dr. Kurlbaum described a surface-bolometer which he had constructed in conjunction with Dr. Lummer. It is cut out of platinum foil whose thickness is 0·012 mm., and possesses the great advantage of very rapidly coming to rest. It is a trustworthy instrument for the measurement of the differences in luminosity of two sources of light.—Dr. Pringsheim described a lengthy series of experiments made in order to determine whether the emission of light by gases is the outcome of mere elevation of temperature, or whether electrical or chemical processes play a necessary part in their incandescence. Sodium vapours were found to yield their characteristic spectral lines and absorption spectra, when passed through a highly heated porcelain tube, only in the case where chemical processes (of reduction) could be ascertained to take place inside the tube. In the absence of these reduction processes, both the emission and absorption of light by the sodium vapours were wanting. The experiments further showed that Kirchoff's law holds good not only for the emission of light resulting from a rise of temperature, but also for that which results from chemical processes, since in all cases the emission spectrum corresponded absolutely to the absorption spectrum.

Meteorological Society, January 12.—Prof. Schwalbe, President, in the chair.—Dr. Sprung exhibited his improved sliding-weight balance, demonstrated its mode of action and extreme sensitiveness, and explained its use in the registration of changes of atmospheric pressure, temperature, and humidity.—Prof. Boernstein spoke of a case of extraordinarily rapid evaporation from both the surface of his body and his clothing, which he had recently observed while on a glacier. He expressed his belief that the evaporation was due to the lesser tension of aqueous vapour, for any given temperature, over a surface of ice as compared with its tension, at the same temperature, over a surface of water. Dr. Assmann put forward the view that the phenomenon was due to the extreme and sudden dryness of the air often observed in elevated regions, and to the powerful effect of solar radiation.—Dr. Andries read a passage from Virgil's "Æneid" which contains a most clear description of a cyclone.

Physiological Society, January 15.—Prof. du Bois Reymond, President, in the chair.—Dr. Max Levy described his experiments on the influence of blood-supply to the skin on the secretion of sweat as seen in the paw of the cat. He found that blood only supplies the material necessary for the secretion. Secretion can be obtained even after complete occlusion of the blood-vessels supplying the glands. After anæmia lasting for 35 minutes the sweat-glands are paralyzed, but can recover their functional activity even after having been deprived of blood for five hours.—Dr. Th. Weyl gave an account of the results of his experiments on animals (pigeons and fowls) rendered immune to anthrax. When anthrax spores were introduced on a silk thread under the skin of these animals, the spores retained their full activity at the end of one day's sojourn under the skin. If kept there for a longer period, they lost some of their virulence, and were found to have become quite harmless at the end of six days in the pigeon, and three or more in the fowl.

Erratum.—In the report of the Meteorological Society for December 1, 1891 (see NATURE, vol. xlv. p. 168) for "maximum and minimum thermometer" read "sling thermometer."

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Cooley's Cyclopædia of Practical Receipts, 2 vols., 7th edition: W. North (Churchill).—Manual of Chemical Technology: R. von Wagner; translated and edited by W. Crookes (Churchill).—The Human Mind, 2 vols.: J. Sully (Longmans).—The Rainfall of Jamaica: M. Hall (Stanford).—The Horse: W. H. Flower (Kegan Paul).

PAMPHLETS.—A New Departure in Astronomy: E. H. (Chapman and Hall).—Hand-book on Viticulture for Victoria (Melbourne, Brain).—Royal Commission on Vegetable Products: I. Ensilage; II. Perfume Plants and Essential Oils (Melbourne, Brain).—Report upon the Condition and Progress of the U.S. National Museum during the year ending June 30, 1889: G. B. Goode (Washington).—List of Institutions and Foreign and Domestic Libraries to which it is desired to send future Publications of the National Museum (Washington).—Te Pito te Henua, or Easter Island: W. J. Thomson (Washington).—Aboriginal Skin Dressing: O. T. Mason (Washington).—The Development of the American Rail and Track, as illustrated by the Collection in the U.S. National Museum: J. E. Watkins (Washington).—Preliminary Hand book of the Department of Geology of the U.S. National Museum: G. P. Merrill (Washington).—Les Odeurs: M. C. Henry (Paris, Hermann).

SERIALS.—Zeitschrift für Wissenschaftliche Zoologie, liii. Band, 3 Heft (Williams and Norgate).—Morphologisches Jahrbuch, xviii. Band, 1 Heft (Williams and Norgate).—Bulletin of the Buffalo Society of Natural Sciences, vol. v. No. 3 (Buffalo).—Records of the Geological Survey of India, vol. xxiv. Part 4, 1891 (Calcutta).

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